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AN EXPERIMENTAL STUDY OF DIZZINESS[1]

BY COLEMAN R. GRIFFITH

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I. INTRODUCTION

A. Historical Setting.[2] Scientific interest in dizziness and vertigo began as long ago as 1795, when Erasmus Darwin[3] first summarized the facts as then known. Purkinje, however, seems to have been the first writer to put the matter to a test in the laboratory.[4] His experiments, which have

[1] From the Psychological Laboratory of the University of Illinois.

[2] A survey of the principal work already done on the structure and functions of the semicircular canals, together with a more detailed account of the history of the psychological problem, is soon to appear.

[3] Darwin, E., 'Zoönomia, the Laws of Organic Life,' 1795 (3d ed., 1801), pp. 327–356.

[4] Purkinje, J., 'Beiträge zur näheren Kenntnis des Schwindels nach autognostischen Daten,' *Med. Jahrb. d. Osterr. Staates*, 1820, 6, 79–125.

since become popularized in the parlor game of whirling with inclined head about a stick, led to a remarkably complete account of the apparent movement of objects in the visual field after rotation. He noted, for example, that the curious movement of such objects always took place in a plane perpendicular to the axis of rotation, no matter how the head was subsequently moved. By way of explanation, Purkinje related all of his observed facts to inertia of the soft parts of the body, particularly the brain. So impressive, it seems, was his demonstration that, for a number of years, no further descriptions were given. In fact, it was not until 1874 that anything further was added to the psychological description of dizziness. In that year, however, Crum Brown[1] and Mach[2] independently attacked the problem in such a way as to show just where the future task was to lie. Crum Brown was the first to use the phrase 'the sensation of rotation,' and since his time frequent references have been made to an 'equilibratory sensation,' to a 'sensation of motion,' and to 'static sensations.' The tendency towards this usage was undoubtedly strengthened by an accumulating body of evidence that a new end-organ discovered, in the years just preceding, in the semicircular canals was closely related to bodily movement and to the maintenance of equilibrium. Crum Brown concluded his investigations in the following manner: "As far as I am aware, the sense of rotation has not hitherto been recognized either by physiologists or by psychologists as a distinct sense; but a little consideration and a few experiments seem to me to be enough to show that it really is so. By means of this sense we are able to determine,—*a*, the axis about which rotation takes place; *b*, the direction of the rotation; and *c*, its rate. . . . In ordinary circumstances we

[1] Crum Brown, A., 'A Preliminary Note on the Sense of Rotation and the Functions of the Semicircular Canals of the Internal Ear,' *Proc. Roy. Soc. Edin.*, 1874, 8, 255–257; 'On the Semicircular Canals of the Internal Ear,' *Proc. Roy. Soc. Edin.*, 1874, 8, 370–371; 'On the Sense of Rotation and the Anatomy and Physiology of the Semicircular Canals of the Internal Ear,' *J. of Anat. and Physiol.*, 1874, 8, 322–331.

[2] Mach, E., 'Physikalische Versuche über den Gleichgewichtssinn des Menschen,' *Wiener Sitz. d. Kais. Akad. d. wiss.*, 1873, 68, 124–140; 'Grundlinien der Lehre von den Bewegungsempfindung,' Leipzig, 1875.

do not wholly depend upon this sense for such information. Sight, hearing, touch, and the muscular sense assist us in determining the direction and amount of our motions of rotation, as well as of those of translation; but if we purposely deprive ourselves of such aids we find that we can still determine with considerable accuracy, the axis, the direction, and the rate of rotation."[1]

Crum Brown's reference to the part played by other sense-departments applies also to another mode of attacking the question, *viz.*, that adopted by Mach. We have already seen that Purkinje sought to explain the facts of rotation by appealing to the inertia of the soft parts of the body and to the sensations resulting therefrom. Early in the history of the semicircular canals the close relation between the canals and the musculature of the body had been noted. Furthermore, a good deal of attention was given to Sir Charles Bell's alleged 'muscular sense.' It is not strange, therefore, that the perception of position and of movements came to be described in terms of sensations from the skin, the joints, the muscles, and the eyes. In 1875 Mach came to the conclusion that although "die Bewegungsempfindungen lassen sich nicht erklären durch die Wirkung der sensiblen Elemente der Knochen und des Bindegewebes, nicht durch die Wirkung der Haut, der Muskel, des Blutes oder des Hirns," nevertheless "ein Mitwerken dieser Factoren bei Erkenntniss der Locomotion nicht vollständig ausgeschlossen werden kann."[2] Later he concluded that of the mental processes involved in the perception the most important is that of vision which, combined with 'skin-sensation' and 'with changing innervations' yields 'a conception of our body as in motion.' Mach doubted that 'special motor sensations exist which proceed from this apparatus (*i.e.*, the canals) as from a sense-organ'; but he thought that the apparatus simply 'disengages innervations after the manner of reflexes.' He regarded as untenable, then, the view that knowledge of equilibrium and of movement is directly mediated by means of the semi-

[1] Crum Brown, A., *Proc. Roy. Soc. Edin.*, 1874, 8, 255.

[2] Mach, E., 'Grundlinien der Lehre von den Bewegungsempfindungen,' Leipzig, 1875, p. 125.

circular canals.[1] In a similar way, Wundt has questioned the existence of special sensory qualities from the canals, since the deaf and dumb acquire "mit Hilfe anderer Sinne, namentlich des Tast- und Gesichtssinnes, eine so vollständige Orientierungsfähigkeit im Raume, dass er, so schwer er den Mangel des Gehörsempfinden mag, den dieses Orientierungsorgans kaum zu bemerken scheint. . . . Eine Organ, das in solcher Weise durch andere vertreten wird, kann aber keine spezifische, nur ihm eigentümliche Funktion haben."[2] Wundt finds, however, that the 'tonische Organ' is functionally related to the other senses. These facts lead him to conclude that "der tonische Sinn kein für sich isolierbares Vorstellungsgebiet umfasst, da er niemals für sich allein Wahrnehmungen des Gleichgewichts und der Bewegungen des Körpers oder gar des Raumes im allgemeinen vollziehen kann, ähnlich wie ja selbst der Sehende niemals durch den blossen Tastsinn Wahrnehmungen äusserer bestateter Objekte gewinnt, bei denen nicht, auch ohne dass er es will, Assoziationen mit den Vorstellungen des Gesichtssinnes mitwirken." The semicircular canals are for him, 'a kind of inner organ of touch.'[3]

Holt has made the first really serious attempt to analyze the experience of rotation. He has found, by experiment, that rotation yields three groups of processes. The first group is made up of 'sensations which proceed from extra-peripheral stimuli,' such as currents of air, light and sound. The second group 'are the sensations from proprio-ceptive organs in joints, muscles and other tissues, which are stimulated by the inertia of the trunk, limbs, internal organs, and even perhaps of the blood, and by their centrifugal moment.' Finally, in the third group are 'the true sensations of motion'; and of the three groups these 'true sensations of motion' are the only ones essential. On examination, however, these sensations prove to be merely the feeling that either the body or objects about the body are in rotation or else they are more or less

[1] Same, 'Analysis of the Sensations' (tr. Williams), 1897, pp. 65–66; 73–77.
[2] Wundt, W., 'Grundzüge der physiologische Psychologie,' (6th ed.), 1910, 2, 507.
[3] Wundt, W., *op. cit.*, p. 508.

parallel to 'innervations of one kind and another.'[1] Recent writers of text-books have met the problem in the traditional way. The usual descriptions of the ear and of the flow of the liquid and of the reflex nature of the responses, together with a description of the field of vision during dizziness, make up the chapters on this 'sense-department.' In its simplest form the psychological problem has been stated thus: Does the excitation of the canals result in a new sensory quality or does the experience gain its character because of the kind and number of other sensory components? Pillsbury states the question in this form and answers it provisionally only. He "speaks for the view that the sensation of giddiness is not a true sensation of the vestibular nerve, but rather a sensation of the alimentary canal, due to reflexes excited by the organs of equilibration" and that "visual sensations, kinæsthetic sensations, and sensations due to displacement of the large visceral organs also aid in keeping the balance and in appreciating the movements of the body."[2] Titchener is persuaded that the canals "act reflexly, without furnishing sensations, or at any rate furnish sensations of little strength, and of a pressure-like kind that blends indistinguishably with the kinæsthetic sensations from the tissues beneath the skin."[3] But he also finds, under other conditions, sensations appearing as a compression or lightness in the head, and a distinct feeling of squeeze in the region of the ears.[4] Warren is still more positive of vestibular sensations of a 'particular quality.' The "sensations of position and sensations of motion" are not only different in quality from one another but "the sensations from the three semicircular canals may also differ in quality" just as local signs are found in the sense of touch.[5] On the other hand, Külpe is quite doubtful of a new sensory quality, although he does admit that 'giddiness' may be taken as

[1] Holt, E. B., 'On Ocular Nystagmus and the Localization of Sensory Data during Dizziness,' *Psychol. Rev.*, 1909, 16, 377–397.

[2] Pillsbury, W. B., 'Fundamentals of Psychology,' 1916, p. 203.

[3] Titchener, E. B., 'Text-book of Psychology,' 1910, 173–182.

[4] *Op. cit.*, pp. 179–180. In his more recent book (A beginner's psychology, 1915, p. 56) Titchener speaks more positively of "the sensation of 'swimming' when the head is sharply jerked, and the sensation of dizziness when we twirl on our heels."

[5] Warren, H. C., 'Human Psychology,' 1919, p. 213.

representing the activity of the canals in consciousness; but "it is difficult to say what the common element in sensations of giddiness is when we have abstracted from the objective disturbance of the coördination of movements and its various concomitant phenomena."[1]

B. The Problems.—It is clear, therefore, that no common opinion is to be found among the writers we have mentioned in this brief historical survey. In fact, we can point out at once two phases of a problem upon which further evidence is needed. In the first place, there is an urgent demand for a more detailed and adequate description of all the processes that arise when a subject is rotated. Furthermore, it will be necessary to classify these processes as regards their importance in the experience of dizziness. That is to say, there are undoubtedly some processes more or less essential to the appreciation of rotation and the selection of these processes must be based upon experimental evidence. This task leads directly to the second phase of the problem. Is there a process or group of processes which stand related to the end-organs in the semicircular canals as the visual processes stand related to the rods and cones in the retina?

Another problem concerns us in this investigation. It springs from a recent clinical use of the term 'vertigo.' We refer to a current distinction drawn by the otologists between vertigo and an ocular effect of rotation commonly known as 'nystagmus.'[2] Now nystagmus is—as they maintain—a simple reflex, and it is not, therefore, subject to change under practice. In a recent article, we have shown that this contention is without foundation.[3] We have shown, on the contrary, that the ocular effects, as well as all the other organic effects of rotation, tend to decrease and finally to disappear under repetition. Our parallel problem here, then, will be to determine the probable modification of the mental results of rotation. In brief, this is our present

[1] Külpe, O., Outlines of Psychology, (Tr. Titchener), 1895, pp. 150 ff, p. 378.

[2] See, for example, Jones, I. H., 'Equilibrium and Vertigo,' 1918, p. 5.

[3] Griffith, C. R., 'The Organic Effects of Repeated Bodily Rotation,' This Journal, 1920, 3, p. 46. See also Griffith, C. R., 'The Decrease of After-Nystagmus during Repeated Rotation,' *The Laryngoscope*, 1920, 30, 129-137.

problem, (*a*) What are the mental processes aroused during stimulation of the end-organs of the semicircular canals and what is their relative significance to the experience of rotation as a whole? And (*b*) what effect will continued repetition and other factors have upon these mental processes?

II. The Mental Effects of Rotation

Our method of experimentation has already been described in a previous article, which was devoted to the bodily consequences of rotation.[1] The method was the method of periodic repetition. In each trial our subjects were revolved ten times.[2] There were ten successive trials taken each day for a considerable number of days. Introspective reports were called for after each trial. The instructions were varied from time to time, as we shall presently designate. We varied our conditions one after another, and we submitted the whole experience to an elaborate fractionation in time. In congruence with previous investigators, we have found that continued rotation results in an experience which is highly complex. We now proceed to an analytic description of the experience on its mental side, first by way of gross description and afterward with detailed analyses.

A. Principal and Accessory Processes.—Of the principal processes that are aroused by rotation we may mention clear kinæsthesis from the eyes, from the head and neck, and from the limbs. Also, there are visceral pressures, coolnesses, and surging or 'welling' qualities. Pressure and even pain may be localized deep within the head and high up in the nasal passages, while all kinds of pulling strains are reported from the region of the diaphragm. Occasionally some of these processes become very clear and reach a climax in the experience of nausea. The processes of this group appear to be aroused by some other cause than the inertia of the body or

[1] Griffith, C. R., this Journal, 1920, 3, p. 20.

[2] The writer wishes to express his appreciation of the services of the subjects. They were V. B. Adams, M. A. Beard, A. R. D'Angelo, W. A. Diesel, M. E. Broom, L. K. Cecil, A. R. Elliott, L. R. Raines, R. W. Wuestermann. H. C. Burleson noted the results from the writer. The writer wishes also to acknowledge his indebtedenss to Professor Madison Bentley for his constructive criticism and supervision throughout the investigation.

the fact that the body is rotated in a resistant medium. On the other hand, and in accord with Holt,[1] we have put into the group of accessory processes such events as are obviously aroused by the inertia of the body, *e.g.*, changing pressure on the buttocks and at the back of the head. In addition, a slightly cool pressure can be localized on the side of the face toward rotation and there is a continuous displacement of the direction of sounds and of lights and shadows.

III. The Factors which Modify the Mental Results of Rotation

All that we have said elsewhere of the organic conditions and results of rotation holds true, as well, for the mental concomitants and effects. Not only is the complexity of mental processes markedly reduced under periodic repetition, but the numerous effects of rotation are just as sensitive to variation in their physical and mental setting as we have found the organic effects to be to variations in the physical and organic conditions.

A. Physical and Physiological Factors.—We are here obliged to insist again that the organic and mental results of rotation are not two different groups but that they constitute a single experience. The organic group provides the conditions for the modifications of mind. We may expect, therefore, that any change in their appearance, as a result of changes in the physical conditions of arousal, will be paralleled by corresponding mental changes. We have found, for example, that variations in the rate and number of turnings, the mode of stopping, the time of day, the length of an interval between turnings, and the general organic set are conditions under which the organic results are modified. The mental effects are likewise modified under the same conditions. For example, the intensity of apparent movement in the visual field and the feeling of 'dizziness' are greatly increased by a sudden stop or by a reversal in the direction of actual bodily movement. These effects are likewise of greater intensity

[1] Holt, E. B., *Psychol. Rev.*, 1919, 16, 377.

when the organic state is abnormal, or when no rest intervals are given, or when the rate of rotation is increased, and so on.

B. Periodic Repetition.—A refined analysis of the mental effects of rotation is made possible only by the proper fractionation of the whole course of events. As we have seen, under rotation mind is exceedingly complex. The mere fact that such gross designations as 'vertigo' and 'dizziness' have delayed further analysis for decades bears testimony to the unusual character of the mental effects produced. We have already pointed out that most of the complexes involved are kinæsthetic and organic, and every introspective psychologist knows how tenaciously such complexes resist final analysis. The very discovery, however, that this complexity does decrease under repetition is an advantage; for the gradual disappearance of one process after another makes possible a fuller description of those that remain. And again, the disappearance of certain processes betrays their presence in a preceding stage. The period of rotation has, for our purposes, been fractionated as follows: (*a*) The period preliminary to any given day's rotation; (*b*) the period beginning with the 'ready' signal and ending with the perception of the first impulse of rotation; (*c*) the period during which rotation is attaining maximal speed, *i.e.*, the first half-turn; (*d*) the period during which the speed of rotation is constant; (*e*) the period from the application of the brake to the inception of after-nystagmus; (*f*) the period from the appearance to the disappearance of the after-nystagmus; (*g*) the period from the end of the after-nystagmus to the next turn; (*h*) the period following the completion of the day's series. We shall now proceed to discuss each of these periods.

(*a*) The period preliminary to the day's rotational series. The description of this period is, of course, quite incidental. There are aroused, however, certain processes which supply a constant factor throughout the experiment and which have, on that account, to be substracted each time from the whole group of processes which appear under rotation. Among them are the tactual processes from contact with the chair and various cutaneous and subcutaneous pressures indicative

of the maintenance of posture. The subject is not in the most comfortable position, for the head is bent forward and, in some cases, the knees are slightly cramped. This position gives rise to achy pulls in the back of the neck and the characteristic fatigue-like pressure about the knees from holding one position for any length of time. It has been found, however, that these processes which arise from the sitting position can be readily detected and left out of account when describing the distinctive mental effects of rotation. The five subjects from whom introspective reports were obtained were instructed to take account of these processes before each day's series and to discriminate them as carefully as possible from the processes set up by rotation.

(*b*) The period beginning with the 'ready' signal and ending with the perception of the first impulse of rotation. The second period corresponds to the preparatory period (Vorperiode) of the reaction experiment. Before rotation begins there is a flood of visual imagery and of kinæsthetic processes. This is the only time at which these processes are at all numerous, for the rotation period itself is peculiarly devoid of imagery so far as our subjects reported. The kinæsthetic processes come as characteristic pervasive thrills, tension about the abdomen and about the chest as in holding the breath.

"I felt this time just as if I had plunged into a tank of cold water. There is a peculiar gripping about the chest and a thrilly feeling deep inside." (P.)

For some subjects these processes mean a fairly complete reinstatement of past experiences in swings, on merry-go-rounds, and especially in high places. All of these processes form a background to the 'feel' of expectation which is carried in part by the verbal kinæsthesis meaning 'what is it going to be like?" and in part by a pressure quality from the throat, diffuse kinæsthetic pressures and pulls from the whole body, meaning a kind of setting or preparation against something about to happen, and a very clear thrilly tingling about the 'pit of the stomach' which radiates in all directions and which is more or less pleasantly toned. The kinæsthetic

processes are localized in the lower limbs, the arms and the buttocks and in the facial muscles, especially about the mouth. After several turnings, the visual and other extraneous imagery almost completely drops out. But the processes constituting 'expectancy' are fairly constant and may reappear at any time. They may even become quite definite in their anticipatory reference.

"Had a very peculiar experience this time. The processes meaning expectation all suggested rotation to the left. Instead I was rotated to the right and there was instituted the same kind of reorganization as is usually experienced only upon stopping." (P.)

Toward the end of the series, however, they become fleeting, more like a suggestion of expectancy, just as the experimenter puts his hand to the chair to begin rotation. As would be expected, all of the processes of 'expectancy' blend into the groups of similar processes aroused just after rotation begins. In case any subject has been previously nauseated or has been sea-sick, this second period is characterized also by clear olfactory imagery. The writer has been surprised at the readiness with which this imagery is aroused as a part of the 'feel' of expectancy.[1] They carry the meaning of disgust and repugnance and are, in some cases, the signal for the arousal of a kind of psychophysical attitude expressed in the exclamation: "I am sure to be nauseated!" In case a subject has become nauseated by the turning, (care was continually taken to prevent it when possible) the olfactory processes form a fairly constant part of the preliminary period through a large part of the series, even though all hint of nausea has long since dropped out. In such cases, it augments the intensity of the 'feeling of expectation,' especially the organic components, and leads to a kind of permanent psychophysical disposition just the opposite to that induced in one of the later periods.

(*c*) The period during which the rotation is attaining maximal speed. Extraneous processes which are similar to

[1] It may be that psychologists would not have failed, as they usually have, to discover olfactory images had they regarded such appropriate occasions as the present.

Holt's first group[1] can be briefly described. It is difficult to estimate just what part they play in producing the apprehension of rotation during the first turns of a series. That it may be a large part is suggested by the fact that, as the series proceeds and other processes drop out, they come to carry almost the whole meaning of rotation. Among the processes found in our experiment are (1) a changed distribution and intensity of pressure from the occiput (head-rest), (2) kinæsthetic sensations from the arms and the lower limbs aroused by resisting the inertia of the body and (3), most prominent of all, a rather clear and intense change in the pressures about the buttocks, probably due to a slight shift in the center of gravity of the body. There is also a light cool pressure on the side of the face in the direction of rotation, lights and shadows begin to move across the field of vision, and there is an apparent change in the direction of extraneous sounds.

Other processes are, however, of more immediate interest. The clearest of these is (4) an intense kinæsthetic play about the eyes. Distinct changes in the quale of these processes can be detected. There is a kind of pressure about the eyeballs which increases in intensity and in distribution and then suddenly gives way, only to return again, and so on in rhythmic fashion. Our observers found it impossible to localize this kinæsthesis on one side of the eyeball rather than on the other. The whole eyeball seemed affected. As the series proceeds, the intensity of this first kinæsthetic flutter about the eyes seems to decrease until, at the end, it has almost, if not quite, disappeared.

"Just as rotation begins there is a decided play of tension or pulling about the eyes." (P.)

"As rotation starts there is a kind of kinæsthetic setting about the eyes." (K.)

"Just as rotation started I felt the usual kinæsthetic flutter about the eyes." (L.)

"The usual rhythmic processes about the eyes hardly ever occur any more. The rotation starts with no more concern than as if I shook my head." (P.)

[1] Holt, E. B., *Psychol. Rev.*, 1909, **16**, 377.

Just before the speed of rotation reaches its maximum, there is aroused in unpractised subjects a tremendously complex mass of kinæsthetic and organic processes from almost the entire body. Constituents can be localized more clearly in the lower limbs, the side of the body toward the direction of rotation, in the arms and the shoulders, all about the chest, in the back and, most clearly of all, in the neck and about the face. During the first periods of rotation these processes arise immediately after rotation begins and reach a high degree of complexity by the time the rotation has come to its maximal speed. The organic or visceral processes then continue in an intensified form from the processes making up the 'feel' of expectancy. This is particularly true where those processes have carried an incipient nauseation. After rotation has been continued for some time these processes do not arise until the end of the second or third turn. All that is left in this period after long practice is the fleeting kinæsthetic flutter about the eyes just before rotation comes to full speed. The swimming 'feel' does not enter into this preliminary period.

"The ocular kinæsthesis at starting has now become but a faint quiver. It disappears during rotation." (K.)

(*d*) The period during which the speed of rotation is constant. The complexity of this period of rotation is at first so great as to make an adequate description almost impossible. It is not strange that the whole group of events making up the period have been dismissed with such words as 'vertigo' or 'a swimming sensation.' As before, the extraneous processes may be profitably eliminated. The pressure about the back of the head arising from the head-rest becomes in this period a clue to the proper position of the head for the head tends to swing in the direction of rotation. The most prominent of the other processes are the succession of lights and shadows and of sounds that flit by. The cool pressure which begins with the rotation, continues throughout this period, but it is of small significance during first turnings. So also with the pressure from the arms and the buttocks. One group of extraneous processes, however, does

play a special part in the experience. For one observer, visual and tactual images of a long, smooth, oily, steel rail, like the smoothness of a piston rod in an engine, carry very largely the meaning of motion.

"There was present this time a kind of tactual imagery of smoothness and oiliness which took up the whole of consciousness. The effects of rotation were not nearly so marked." (P.) Sometimes the kinæsthetic and organic qualities so completely disappear that the subject regards himself as standing still and the visual shadows and the auditory qualities as floating past him. On these occasions, the after-effects are greatly reduced—especially the organic complexes. On the other hand, there is a type of visual imagery which carries the meaning of a dizzy height around which the subject is being whirled and from which he is looking down into a dismal abyss. Such imagery was usually accompanied by a marked increase in all the muscular and visceral effects of rotation; the after-nystagmus was longer and the whole experience much troubled. The appearance of these or of any other imaginal materials of a similar kind was, however, rare.

"Frequently today had visual imagery of various moving objects and some verbal kinæsthesis in counting the number of rotations, but such processes are generally rare." (P.)

As the daily sessions continue, all the effects become greatly reduced in clearness, duration, intensity and qualitative characteristics. As this reduction takes place, there is a corresponding increase in the part that the extraneous processes play. Finally, the whole meaning of rotation is carried by them, although the kinæsthesis in the neck is still clear. This latter gives the meaning of some kind of movement and it might be found that, with the other processes eliminated, it is sufficient to carry the meaning of rotation. It still retains its characteristic distribution on the side of the body toward rotation, as do also the vague remnants of the processes that were formerly so clearly localized about the arms and legs. It can safely be said, however, that as rotation continues, the perception of it comes to be carried more and more in terms of the extraneous processes rather than in terms of the

remnants of kinæsthesis. The extraneous processes are almost the only ones which maintain their intensity unchanged whatever the amount of practice. As the other processes begin to drop out, these processes come to play a larger and still larger part until at last they give almost the entire perception of the direction of rotation.

"Rotation seems to be carried almost entirely in terms of the extraneous processes today." (K.)

"The perception of movement was carried this time very largely in terms of tactual, thermal and auditory processes." (K.)

"Was quite sure this time that, in addition to the usual kinæsthesis about the neck and face, the only processes which carried the meaning of the direction of rotation were the tactual processes—coolness (of air), and auditory qualities." (P.)

"The perception of rotation carried very largely in terms of coolness, etc., today. I believe that if they were not present, I should not be able to distinguish between rotation and linear movement. The neck and the face kinæsthesis might carry the meaning; but I doubt it." (M.)

All of the organic and kinæsthetic processes which were aroused during the starting period run their course with a good deal of intensity as rotation continues. The rhythmic kinæsthesis about the eyes becomes almost painful. By the time the first turn is completed the whole body is involved. The visceral qualities may become violent. The somewhat diffuse thrilly qualities of 'expectancy' suddenly become localized into a kind of drawing pressure about the 'pit of the stomach' and gradually rise to a point just below the diaphragm where part of the intensity is lost in favor of a pulling pressure throughout the diaphragm and in the abdominal walls and a part in favor of tension about the colon and the anus. From the diaphragm there ascends into the œsophagus, a heavy pressure which causes a sort of gulping. (P.) Just as the pressure in the diaphragm comes to full intensity, a very diffuse warmth spreads over the whole body, perspiration starts, a bad taste appears in the mouth, there is an excessive

flow of saliva, an achy spasmodic pressure about the stomach and unless rotation is stopped, a 'nauseated feeling' spreads over the whole body and vomiting may take place. (P, N, L, K.) In the meantime, a lumpy pressure or swelling has arisen deep within the chest and has ascended to the head, where it may spread out as diffuse achy pressure, or it may become localized about the side of the head toward rotation or over the eyes. (P, K.) There then appears a kind of swimming within the head and with this feeling the individuality of all the other effects is lost. Even the very clear tensions and pulls in the arms, neck, back, legs, face, and especially the eyes, seem to lose their specific character and become fused into the 'swirling sensation.' To quote from the records:

"There was a kind of stuffy ache in the stomach and in the higher viscera, and a kind of tenderness and pressure at the anus." (P.)

"There was an unpleasant kind of pressure low in the trunk and a periodic pressure and pulling about the anus." (N.)

"Found this time a kind of tension about the anus and similar in quality to that about the chest." (P.)

"Felt decided tension in the diaphragm, the abdominal wall and up through the chest, becoming less clear about the throat but very clear at the base of the brain and in the head. There was a distinct 'swelling' pressure in the head." (K.)

"Felt this time a kind of spasmodic pressure or pulling at the lower end of the œsophagus and in the stomach region. Also distinct pressure about the anus." (P.)

"There was a general organic thrill of expectancy just before rotation began. This became definite but of same quality; after rotation started was localized high in the viscera. After two or three revolutions it became fused with other processes from the legs, arms, eyes, etc., so that it was hard to isolate them any longer. Just then a swimming quality developed. Later a distinct pressure bobbed up within the lungs near the heart and there was a slight 'choking' complex." (K.)

"Vertigo seems to be entirely a matter of a peculiar fullness in the head and a pressure localized almost painfully about the nose and in the eyes." (P.)

"Very peculiar change this time, Previously it has been hard to single out the items because they seemed so confused and the peculiar whirling sensation in the head which seemed to depend upon an odd kind of pressure and lightness confused me. This time, however, the whole experience seemed broken up. There were many specific items to be found but the swimming sensation was no longer present." (M.)

"This time the whole experience was again broken up save just at the last when I seemed to lose control of myself. Confusion set in and there was immediately a fleeting return of that whirling sensation in the head." (P.)

"This time, just as the confusion and whirling sensation began, I set myself as definitely as I could against it. The setting was carried mainly in terms of pressure in the feet and buttocks as if I were trying to hold back an automobile. With this setting the confusion disappeared, as did also the swimming, and I was then able to describe the several single items." (N.)

"Seems as if there was a large fluid mass in the head which was whirling around very rapidly and which feels very much as a flowing stream of water looks. This became very confusing until I set myself against it. It then seemed to break up into pressure in the head and also a lightness on one side, and also pressures and pulls all about the face and neck and especially in the eyes." (P.)

It is clear that the swimming sensation is not a permanent part of the experience of rotation. Within a few turns it has entirely disappeared and does not return again. In fact, as rotation continues from day to day, all of these events decrease in intensity and duration, and in the end almost all of them disappear.

"I found the usual processes present this time but again all of them were barely noticeable. There was slight kinæsthesis about the eyes as rotation started, and then in the neck and arms and a little about the chest (slight tendency

to hold breath yet). There were only very meager pressure qualities from the viscera and in the head." (P.) "Tension about the rectum, abdominal wall, some internal pressure and chest kinæsthesis all of low intensity are the items in a kind of setting or attitude of tension which is present during each period of rotation now. This is coupled with the neck-and-arm and face tension. Both of these are different from those qualities which seem like an internal rising coolness which becomes pressure in the neck and in the head and nose." (K.) "No particular processes today. Everything has become but a vague hint of what it used to be." (M, N.)

The visceral qualities change a little as practice continues. It is as if they were exposed to the air and a light cool breeze were blowing upon them, giving a very curious but delightfully pleasant thrill. This thrilly quality lasts but a turn or two and then rises and changes into a weak and diffuse pressure about the diaphragm. Finally even this thrilly quality drops out and all that is left is a dim pressure about the diaphragm.

"A very diffuse head and stomach pressure as from the presence of a slightly distended balloon seems to be about all that is left of the former organic complexity." (P.)

"The eye kinæsthesis has become reduced to vague hints.' (K.)

"Dim organic processes pretty well diffused through the diaphragm and lower œsophagus were about all this time." (K.)

"Some kinæsthesis about the chest, as in holding the breath, very slight organic pressures and pullings in the visceral organs, and some pressure which arose and spread out through the diaphragm and about the lungs." (M.)

With the disappearance of some of these more prominent items, it is possible to attend to a new group.

"Behind the group of clear and distinct processes which formerly seemed to make up the core of vertigo, is a dim, diffuse, undifferentiated mass of processes no one of which seems to rise above another, but on various occasions, such as stopping, the mass seems to shift in color or size or position, or something of that kind. This shift is very subtle, like the

changing curtains of an aurora borealis. When one watches an aurora it seems to change; but what is changed or at which place cannot be described. You know it has changed. So here, these background processes change as rotation starts and then, as full speed is attained, there is a settling back or retreating until the stopping period. Occasionally definite processes shoot our like a streamer from the aurora. These processes are always pulls or tensions or pressures from the trunk near the diaphragm and often there is a feeling of warmth or of coolness from the visceral region. The warmth is localized higher up and more peripheral than the cold. The affective quality is variable." (P.)

Just what neural conditions are responsible for this background of undifferentiated processes is hard to say although the work of Camis[1] and others suggests that the stimulation of the canals may have a more definite effect upon vascular and motor conditions than has as yet been determined. Historically, of course, the notion that there are movements of the viscera and of other soft parts of the body which account for the perception of movement has been a common one. Four of the subjects reported this organic shift just after the major complexity of vertigo was gone. It seems to have dropped out during the later trials.

A third group of organic processes consists of a very dull kind of pressure or 'swelling' which seems to arise out of the trunk and ascend into the head like the raising of the level of water in a tank. These are the processes which come most directly from the disappearance of vertigo. As rotation continues, this pressure becomes more intense and clear, being localized sometimes about the ears, sometimes over the eyes, but generally deep within the head. Along with this pressure there is also a decided pressure from the upper part of the nose, and the internal part of the nose feels raw—just as if one were about to suffer a recurrent nose-bleed.

"Found this time that the pressure about the upper part

[1] Camis, M., 'Contributions à la physiologie du labyrinthe; Observations ulterieures sur les phenomènes vaso-moteurs,' *Arch. ital. de biol.*, 1911, 56, 277–288; "La glycosurie consecutive à la destruction des canaux demi-circulaires chez les chiens,' *ibid.*, 289–300.

of the nose, as in nose-bleed, was more clear and distinct. The nose became tender and raw." (P.)

"The pressure rising at the back of the head became heavy and achy this time as rotation continued." (K.)

"The headache I had at the beginning of the turning was changed into a distinct pressure quality high up in the nose." (L.)

"Pressure in nose this time and also a kind of fresh feeling of tenderness as if one had just douched the nasal passages with salt water." (P.)

Here again, the effect of practice is to decrease in intensity and duration all of the processes concerned. Other things being equal, the decrease in the intensity of the processes aroused by rotation proceeds without interruption until all that is left during the actual rotation-period is the rather diffuse kinæsthetic quality from the face and neck.

"All qualities are becoming less and less intense and shorter, save those from the neck and about the face." (N.)

These processes, which were not at all clear, gave promise of disappearing altogether. As practice continued they arose later and later in the turning period until, at the end of the series, they appeared only when the rotation was half over.

"Neck-kinæsthesis and nose-pressure not apparent this time until the rotation was half over. None of these processes ever appear any more until late in the rotation-period." (P.)

"Did not get anything clear until rotation was almost over. Then only the vaguest hints of pressure from the viscera and kinæsthesis from the face." (M.)

There was no change in the quality for rotation to the right or to the left save that the kinæsthesis was always clearer on the side toward rotation, the pressure from the head appearing to change location with the direction of rotation, especially late in the series. Just what kind of a change this was could not be determined. Also all processes seemed to be a little clearer during rotation to the right than to the left, with two subjects.

"All processes seemed to be a little clearer and more intense during rotation to the right." (P.)

"All processes were less clear and less durable this time." (Rotation to the left.) (K.)

(*e*) The period from the application of the brake to the inception of after-nystagmus. The most startling experience resulting from rotation occurs during this period. At first the period is the most upsetting and ordinarily it is responsible for the production of nausea. There is a shift in the pressure at the back of the head, a decided change in the quality of the pressure about the buttocks, and the extraneous visual and tactual processes drop out. A painfully intense kinæsthesis appears localized about the eyes and then suddenly gives way to a series of rapid fluctuations similar in intensity (although more rapid) to those described above.

"The rhythmic appearance of kinæsthesis in the eyes does not appear until stopping has been completed and comes as a climax to that long intense process which starts just as stopping starts and which carries the meaning of the deflection of the eyes in the direction of rotation. The feeling of rotation in the opposite direction and the jumping of the visual field does not begin until this rhythmic kinæsthesis begins." (P.)

"A very distinct change in eye-kinæsthesis was felt as stopping began. The eyes were directed toward the side opposite to rotation. They gradually and involuntarily moved over to the extreme opposite and then began the jerking movements. The shift in the kinæsthetic processes about the face and neck was very clear this time." (K.)

"Just as rotation stopped there was intense kinæsthesis from the eyes, as if they were being pulled in the direction of rotation and, just as stopping was complete, the kinæsthesis reached its maximal clearness and intensity, becoming almost painful, and then (as the after-nystagmus began) it gave way to a rhythmic variation in intensity." (L.)

The intense kinæsthesis carries with it the meaning of movement of the eyes in the direction of the preceding rotation. Even when ocular kinæsthesis has about disappeared, this one process remains. The perception of rotation in the reverse direction never begins until it gives way

to the rhythmic nystagmus movements. The most outstanding fact is the total reorganization that occurs in the musculature of the body. There is a sudden shift in these processes, the more intense processes giving way to a feeling of relief or of release and other muscles are localized as the source of intense pressure.

"The stopping period is full of kinæsthetic pulls and tensions and pressures localized mainly in the eyes and in the neck." (P.)

"I caught very clearly this time the shift in the kinæsthetic set during the stopping period. The kinæsthesis during actual rotation was not intense, but was clear, and then, just as rotation stopped, there was a kind of mental pause followed by a shift in the locus of pressure. It seemed to go to the other side of the body, especially in the arms and the shoulders." (K.)

"The change which takes place during stopping is more than a resetting. It is like a kinæsthetic and organic shifting as if some change took place in the position of the organs or substance internal to the skin and muscles. Sometimes this change is clear and sometimes it can be reported only as something different—a kind of organic and kinæsthetic background which seems to change tint-value, as it were. Sometimes 'welling' and pressure are distinguishable; and then again analysis is baffled." (P.)

This shift or change in locus is as temporary as are the other effects of rotation, and soon (under repetition) the kinæsthesis in the eyes is the only clear process left. The pressure in the head is very apt to increase temporarily. The eyes are opened as soon as rotation ceases and the whole effect of rotation counter to the real rotation is carried to the field of vision. The description of the field of vision we have found to make up a large part of previous descriptions. If the eyes remain closed, the kinæsthetic processes about the eyes and the fast disappearing kinæsthetic and organic processes in the body at large carry the entire meaning of rotation. After much practice, the complexity of the stopping period becomes less, but it is the last complex to give way.

(*f*) The period from the appearance to the disappearance of the after-nystagmus. The rhythmic kinæsthesis from the eye, which begins as soon as rotation comes to a full stop, continues in the initial turnings for some 20–25 secs. In fact, it is the only thing that is peculiar to this period. The pulls about the eyes are almost painful at first and absolutely uncontrollable. Even after some practice, when a subject has learned to control himself during actual rotation, the confusion persists during this throbbing movement of the eyes when the individual comes to rest. In some cases it carries, for a time, the perception of rotation in the opposite direction; but ordinarily there is only a fluency or a jumpiness of the visual field in a plane corresponding to the plane of rotation. If the eyes are kept closed, the perception of rotation is always present. The kinæsthesis from the arms and shoulders, which may be very intense at first, persists for a few seconds; but that from the neck and face usually continues on with about the same clearness. There are no organic processes peculiar to this period. Whatever is present is but the persistence of sensations set up during the actual rotation. The kinæsthesis about the eyes grows less and less and it finally disappears. The jumpiness of the visual field disappears at the same time.

"The visual field jumps no longer. It is a little blurry and confused for a moment and then clears up suddenly about the same time as the general release in tension is felt about the face and eyes." (P.)

(*g*) The period from the end of the after-nystagmus to the beginning of the next turn. We have found that the pressure from the head and from the upper part of the nose continues almost unabated during the course of the after-nystagmus. Following the last eye-movement there is a period of a few seconds in which these processes continue and then suddenly there is a complete relaxation of the tenseness about the face, the jaws, the eyes, and in the upper part of the neck. Coincidentally there comes a peculiar and unique change in the feelings about the head. For example:

"I paid particular attention to the after-period this time.

There is a great deal of kinæsthesis—much more than I had supposed—about the face, and the release of the tension was very marked. The pressure in the head which began about the second turn and continued through the nystagmus-period, took a decided change. There was a clear and decided decrease in pressure as though something had given away lower down and the level of pressure had suddenly fallen as the level of water falls when an opening is made at the bottom of a dish. The disappearance of pressure in the nose left the usual 'raw' feeling." (P.)

"The pressure in the head begins about the second rotation. It is quite intense during the stopping period and then, a few seconds after nystagmus, there is a general release of tension all about the face and a lowering of the level of pressure in the nose which leaves a salty 'raw' feeling. The whole experience is of something falling down deep within the brain and neck, which leaves a kind of fresh coolness as opposed to the former stuffiness." (P.)

The change that takes place here, however, is not different from the changes we have found from the first, so far as regards the effect of practice. That is to say, at the end of a series this general lowering of pressure and release of tension drops out. We have even found that the initial pressure itself does not arise.

A similar modification of pressure and tension may be induced at any time, if the position of the head is changed, during the rotation or at any prior time, to exactly the 'release' which we have described. It is also possible to reestablish in this way the 'vertigo' or 'sensation of swimming.'

"As I bent the head forward, there was a distinct change in pressure, the pressure disappearing as well as the kinæsthesis about the face and neck, save in the eyes where there was that characteristic fleeting, hazy, filmy kind of pull in all directions as if the eyes didn't know where to go. This all gave a feeling of light-headedness which is almost like vertigo. As the pressure leaves the head and neck region, the head seems wobbly and 'lightly propped.' It is more of an effort to keep the head in one position than to move it." (P.)

"Felt the same lightness in the head again after changing the position of the head following rotation. The 'swimming' seems to result from an absence of the pressure, which is usual up to this time. There is also an absence of kinæsthesis in the neck which leaves the head with a feeling that it can wobble all over. There is also a peculiar kinæsthesis about the eyes, as if they were going to move in all directions at once. The visual field looks blurry for a moment, like the gravel behind a fast moving train." (P.)

"The feeling of lightness and of dizziness, after rotation is over, is marked mainly by the absence of pressure in the head and nose but with the presence of a strange kind about the eyes, as if the eyes were trying to move in all directions at once and the result is a kind of blankness or grayness of the visual field. The visual field looks as it does during the onset of nausea or after one has suddenly arisen." (K.)

"As I bent head forward this time I felt a kind of creepy coolness arising internally, deep in the chest, moving rapidly through the neck to the brain and then a darkening of the visual field and a sense of confusion and light-headedness." (N.)

All of these things seem to mark the final cessation of vestibular excitation, whatever it may have been. Whenever the "release" described takes place, the subject is ready to change his position or to be rotated again without expecting any of the previous disturbances to carry over. Furthermore, the subject usually takes at this time a deep breath—the first since rotation began.

(*h*) The period following the completion of the day's series. After the first few day's practice, subjects reported that it was several hours before they completely recovered from the effects of rotation. This is natural in the case of those subjects who became nauseated, for such a severe organic disturbance could not but affect the organism for some time. Interest lies, however, in the case of those individuals who were not nauseated and who nevertheless reported that they felt as if they had been engaged in hard manual labor. Some felt as if they had just lifted a heavy weight;

others as if they had run up a long flight of stairs, and still others as if they had recovered from sickness and were still weak. The reference was made by almost all subjects that the feeling was one of fatigue in the whole body and especially in the legs. Occasionally there was a diffuse light-headedness. This effect tended, along with the others, to become less prominent as the series proceeded.

"The final processes today were about as usual. There is always a kind of draggy pull at the base of the brain which seems gradually to accumulate during the series. There is a tingling of the feet and elsewhere, as if the body had been for some time under tension and strain. These processes generally last about a half hour. The head seems heavy, as if ready to fall in any direction at any time and there is a tired feeling in the neck. These processes are not nearly so intense or clear, however, as they were early in the series." (P.)

The foregoing recital confirms our statement that the experience of rotation is exceedingly complex. Evidence has been adduced to show that there is not a new quality which can be called the 'sensation' of rotation. It is proper only to speak of the perception of rotation, the perception resting upon a host of kinæsthetic pressures, pulls and tensions from all over the body and of pressures and pulls from the viscera, pressures and lack of pressure from the head, and so on. When all of these events reach a certain degree of complexity they seem to fuse together into what has been called 'vertigo' or 'dizziness.' Our analysis goes to show that neither of these terms refers to a single item; but rather to the state of mind regarded as a whole. As one becomes adjusted to rotation, this total state disappears and the whole course of events suffers disintegration. It is then possible to reinstate the 'swimming' components and so to identify the individual items in the whole group. Finally, we have found that the mental results of rotation behave in the same manner under repetition as do the organic results. That is to say, the processes which we have designated as principal processes tend to decrease in complexity and in intensity and clearness

TABLE I

DISTRIBUTION IN GROUPS OF 50 ROTATION-PERIODS OF THE VARIOUS MENTAL PROCESSES AROUSED BY ROTATION. SUBJECT *P.*

Mental Qualities		Tendinous Strain						Muscular Pressure						Fatigue					
Groups of 50 Rotation Periods		1st	2d	3d	4th	5th	6th	1st	2d	3d	4th	5th	6th	1st	2d	3d	4th	5th	6th
Kinæsthesis referred to	Eyes	45	41	23	15	14	8	47	40	32	16	11	5	11	7	3	0	0	0
	Face	40	31	18	12	7	7	49	31	23	17	8	2						
	Neck	46	45	21	18	14	11	45	36	27	13	7	1	35	16	11	10	1	1
	Arms	47	31	19	18	12	3	49	30	22	17	6	4						
	Back	32	24	14	7	4	1	41	33	19	4	0	0						
	Legs	30	19	12	6	5	3	27	16	4	0	0	0	13	6	3	0	0	0
	Shoulders	27	23	7	2	1	0	20	17	6	1	0	0						
	Chest	31	15	3	0	0	0	45	32	19	11	6	6						

Groups of 50 Rotation Periods		1st	2d	3d	4th	5th	6th
Imaginal processes referred to	Vision	5	1	0	1	0	0
	Touch	3	8	3	0	0	0
	Kinæsthesis	5	3	1	0	0	0
	Smell	10	4	0	0	0	0
Extraneous processes referred to	Taste	6	2	0	0	0	0
	Hearing	2	3	8	10	17	23
	Vision	13	10	11	15	14	16
	Touch	8	10	17	23	27	33

Mental Qualities		Cold and Warmth						Pressure						Pain + Pressure						Welling Pressure					
Groups of 50 Rotation Periods		1st	2d	3d	4th	5th	6th	1st	2d	3d	4th	5th	6th	1st	2d	3d	4th	5th	6th	1st	2d	3d	4th	5th	6th
Organic and Vascular Processes Referred to				(Cold)																					
	Anus								8	2	0	0	0												
	Bowels	16	3	0	0	0	0	41	16	3	0	0	0												
				(Warmth)																					
	Stomach	18	2	0	0	0	0	47	12	0	0	0	0												
				(Warmth)																					
	Diaphragm	6	2	0	0	0	0																		
	Chest							27	18	14	7	6	0							25	17	11	10	4	0
	Esophagus																			31	16	5	3	3	1
	Neck																			42	12	14	11	3	2
				(Cold)																					
	Head	4	2	0	2	2	1							19	14	11	7	6	6	49	37	31	23	13	10
	Nose													16	12	7	6	6	4	45	29	21	18	16	12
	Skin	42	22	10	0	0	0																		

and finally to disappear. The accessory processes, on the other hand, tend to increase in clearness and so to carry the perception of rotatory movement.

That there is a decided decrease in the principal effects may be further deduced from Table I. It will be seen that, whereas the processes directly dependent upon rotation decrease in number as a series progresses, the extraneous processes actually increase in number, a result in accordance with the introspective reports that the perception of rotation comes after long practice in turning to be carried almost entirely by these processes. Furthermore, upon examining Fig. 1,

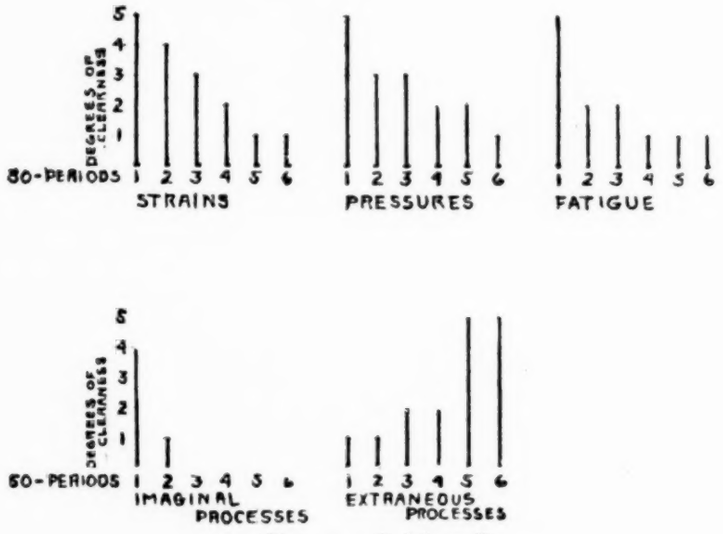

FIG. 1. Subject *P*

it will be observed that the clearness of the essential processes likewise decreases whereas the clearness of the extraneous processes increases. In a similar schematic way, it could be demonstrated from introspective reports that the intensity and duration of both of these groups decrease and increase respectively. We have already referred to the fact that, toward the end of the series, the kinæsthetic processes appeared only after the fourth and fifth rotations and we have also at hand ample evidence of the reduction in duration.

C. The Direction of Attention.—The effect of changing the direction of attention was demonstrated in the following manner. Two subjects, *I* and *J*, were instructed to regard as attentively as possible a definite fixation point on alternate

trials. During the remaining trials they were given a problem in 'mental' multiplication to solve, while looking passively, only, at the point of fixation.[1] In these experiments it was thought that this alternation of the conditions might confirm or condemn the otologist's assertions that whatever decrease takes place is the result of "voluntary gaze-fixing."[2] It can be seen from Table II. that the effect of holding the fixation mark tends to leave at a higher level the duration of the nystagmus, the number of movements, and the time of apparent movement. As regards the *amount* of decrease, it appears that, for subject *I*, the decrease during the series (compare first and last values) is greater in the problem series than in the steady fixation series. The problem series gives less decrease, however, in *J*'s case for the distraction of the problem.

TABLE II

Fixation	*I*			*J*		
	T	No.	A. M.	*T*	No.	A. M.
Av. first 50 trials	14.8	23.4	11.1	15.9	35.4	10.9
Av. last 50 trials	12.2	18.5	10.7	12.7	31.2	8.5
Decrease	2.6	4.9	0.4	3.2	4.2	2.4
Problem	*T*	No.	A. M.	*T*	No.	A. M.
Av. first 50 trials	13.2	18.3	10.3	12.3	26.0	11.2
Av. last 50 trials	9.0	10.4	8.9	9.9	20.5	8.8
Decrease	4.2	7.9	1.4	2.4	5.5	2.4

It is not clear, however, just what is involved in attending to a fixation point. From introspection it was discovered that the kind of attention present in this case was accompanied by a great deal of kinæsthesis from the face, especially from the forehead, neck and eyes. The subjects 'stared'

[1] The problems were chosen experimentally and were designed to keep the subject busy as long as the after-nystagmus continued. Care was taken, however, not to make the problems so difficult as to confuse the subject and thus to defeat the purpose of the trial.

[2] See Manual Medical Research Laboratory, Wash., D. C., 1918. Also Fisher and Babcock, *J. Amer. Med. Ass.*, 1919, 72, 780; Griffith, C. R., 'Concerning the Effect upon Nystagmus of Repeated Rotation,' *The Laryngoscope*, 1920, 30, 22–25.

with all their might in an effort to overcome the confusion resulting from the nystagmus. It is not possible, therefore, to attribute the decrease in the intensity of the ocular effects of rotation during practice to the art of 'gaze-fixing.'

D. The effect of the psychophysical determination. The facts we have just presented are further illuminated by the results issuing from a changed mental determination. The introspective records have already suggested the influence of this mental pre-condition.[1] We have seen that the determination induced by certain imaginal processes profoundly influenced the course of the subsequent processes. We have also seen that, under the influence of the instruction given above, the effects were increased in intensity. Further instructions were now given to the effect that the subject should on some occasions induce the 'staring' attitude and on other occasions the resisting attitude of the whole body. In this way it was discovered that the setting of the whole organism against the confusion of rotation served to check the confusion and to shorten the duration of the processes, a result quite contrary to that obtained by staring at a fixation mark. For example:

"I tried to inhibit nystagmus this time. Attitude of resistance was carried in terms of tension about the legs, buttocks, arms, and back. The whole body was set in resistance. Found that I was quite successful,—success being carried in terms of greatly reduced confusion." (P.)

During a group of thirty trials Subj. *P*'s averages for trials involving no setting were 20.1, 8.5, 20.6, and for trials involving the resistance of the whole body 10.4, 5.4, 10.4. It seems, then, that such a psychophysical preadjustment, while not accomplishing much early in a series, may later reduce very materially the time of nystagmus, the number of movements, and the time of the apparent movements.[2]

E. Visual Factors.—Attention has frequently been called to the effect of changes in visual factors on the appearance of

[1] See p. 99.

[2] This sort of decrease is not to be confused, however, with the decrease that issues directly from practice. A subject may reduce the time of nystagmus considerably by setting himself against it, but the decreases last only as long as this attitude is maintained. For example, such artificial decreases do not reappear the next day.

nystagmus. For instance, Bárány says that nystagmus may be completely inhibited by looking fixedly in the direction of the slow component of the movement. We are here interested, however, not in the matter of voluntary inhibition, but in the ways in which the appearance of the mental effects of rotation may be modified by changes in the visual conditions.

1. Character of the visual field during rotation. We shall first discuss the effect of the character of the visual field during rotation. Subj. *M* was rotated in the regular manner, but on alternate trials he was blindfolded during the period of rotation while during the remainder of the trials his eyes were left open. It was found (see Table III.) that the nystagmus was greatly decreased in time when the eyes were kept open during rotation and that the amplitude of the movements was smaller. Subj. *M* also reported a greatly decreased amount of dizziness when the eyes were open and during the early part of the series the nausea and other organic disturbances were less intense.

TABLE III

Trials	Closed			Open		
	T.	No.	A. M.	T.	No.	A. M.
First 50	17.6	31.0	15.9	14.5	21.3	13.2
Second 50	14.2	23.0	13.9	11.4	12.6	10.6
Third 50	10.8	17.4	10.8	6.7	7.5	6.9
Fourth 50	9.1	12.3	9.2	4.6	4.4	4.6

2. In the second place, we shall discuss the effect of the character of the visual field after rotation, or, in other words, the effect of the mode of fixation. Three variations in the mode of fixation were required of our subjects. These were (*a*) the effect of near-fixation versus far-fixation, (*b*) the effect of no-fixation, and (*c*) the effect of lateral fixation. In the first case, the subject was allowed in part of the trials to fixate a point about one hundred feet distant, *i.e.*, well beyond the proximal point for parallel axes, and in the other part of the trials the subject fixated a point six inches distant. Under these conditions, it was found that the time of nystagmus, the number of eye-movements, and the time of apparent

movement were approximately twice as great during far-fixation than during near-fixation. See Table IV. This difference appears not only in the groups of five-day periods but also for the average of the whole series. The most pronounced difference is found in the number of the eye-movements.

TABLE IV

Trials	Far Fixation			Near Fixation		
	Time	No. of Mvt.	App. Mvt.	Time	No. of Mvt.	App. Mvt.
Av. first 50.....	15.8	30.5	13.8	7.9	12.1	7.5
Av. second 50.....	10.1	18.9	9.7	5.7	7.7	5.5
Av. third 50.....	5.6	9.0	5.6	2.9	3.5	2.9
Av. fourth 50.....	3.2	4.7	3.2	1.2	1.2	1.2

b. As regards non-fixation, three variations in method were used. In the first case, Subjs. *R* and *S* were rotated as usual, but their after-nystagmus was observed while they were wearing lenses of 20 diopters. In the second group, Subjs. *Q* and *T* used lenses, but they were also stopped facing a homogeneous surface of grey paper. In the third case, Subjs. *D* and *G* were rotated as usual, but the nystagmus time was obtained by observing from the side as a piece of cardboard 3 x 6 in. was held before the eyes of the subject. In all these cases, the effort was made to eliminate, so far as possible, all fixation. The results are so nearly alike that they will be treated together in Table V.

An examination of the table shows that the initial values are much larger than in the case of 'normal' vision. In fact, the average initial time of nystagmus (not including Subjs. *G* and *V*, who took this series after a long series under other conditions) for these subjects is 41.0 sec. which is over twice as large as the initial values for usual fixation. In other words, this value is as much larger than the value obtained under normal conditions as the value obtained under normal conditions is larger than that obtained under near-fixation (at 6 in.). The initial values obtained from Subjs. *G* and *V* are pertinent. Both of these subjects had had long previous practice. Subj. *G*, *e.g.*, had lost all ocular movement under

TABLE V

DECREASE IN TIME (SEC.) OF NYSTAGMUS AND NO. OF OCULAR MOVEMENTS FOR SUBJECTS *D*, *Q*, *R*, *S* AND *T* WHILE WEARING + 20 DIOP. LENSES

Trial	Time of Nystagmus													No. of Movements										
	Subjects													Subjects										
	D	*D* Con.	*D* Con.	*Q*	*Q* Con.	*Q* Con.	*R*	*R* Con.	*R* Con.	*S*	*S* Con.	*T*	*T* Con.	*D*	*D* Con.	*D* Con.	*Q*	*Q* Con.	*Q* Con.	*R*	*R* Con.	*R* Con.	*S*	*S* Con.	*T*	*T* Con.
1	18.0	10.8	1.9	45.9	20.9	18.2	35.0	32.3	22.2	33.0	26.2	50.0	30.1	13.5	7.6	1.9	45.4	27.4	24.5	37.2	28.6	17.0	53.7	43.8	88.5	60.9
2	17.8	10.9	1.2	44.3	21.7	20.1	35.9	26.6	17.4	32.0	25.4	41.1	27.6	16.9	10.6	1.2	47.8	32.0	26.5	38.6	17.1	12.7	52.1	39.3	89.4	57.5
3	17.3	8.1	0.4	44.6	19.9	18.2	33.0	25.4	16.1	31.0	25.2	39.0	25.8	14.7	7.5	0.4	46.7	24.7	25.3	29.1	22.0	12.8	50.2	37.9	73.2	45.0
4	14.0	6.2		43.4	20.8	19.0	39.6	29.1	12.0	36.2	20.7	39.1	21.8	9.7	4.8		49.6	29.3	26.5	32.6	24.1	7.2	58.3	32.9	73.7	51.2
5	14.5	7.5		35.1	19.9	18.2	39.2	27.3	6.0	29.8	17.8	40.5	24.8	12.2	7.2		43.1	26.8	24.8	34.5	24.7	4.5	48.9	25.7	76.5	55.8
Av.	16.3	8.7		42.6	20.6	18.8	35.6	28.1	14.7	32.4	23.0	41.9	26.0	13.4	7.5		46.5	28.0	25.5	34.4	23.3	10.8	52.6	35.9	80.3	54.5
6	9.3	6.1		29.5	25.6	18.9	35.0	25.5		25.4	17.7	31.4	24.8	7.8	5.3		37.2	29.4	26.3	29.9	25.5		45.0	29.7	64.1	52.8
7	11.3	4.3		31.3	19.6	19.7	36.5	24.3		28.1	22.1	31.8	22.9	8.5	3.6		34.7	24.0	29.6	16.0	16.9		50.7	34.1	63.9	47.8
8	8.6	4.1		26.2	21.3	19.0	29.5	16.2		31.3	17.4	35.0	25.5	8.1	4.7		27.5	28.8	24.9	23.4	10.7		48.0	27.8	65.5	52.0
9	10.0	2.6		32.6	21.3	18.3	31.2	17.5		33.2	16.2	38.7	20.4	8.8	2.7		35.9	27.3	25.1	27.8	11.9		49.8	24.1	69.4	43.5
10	9.5	3.9		27.7	20.8	16.1	31.6	20.2		27.2	15.3	27.8	18.7	6.7	3.1		39.5	26.4	22.5	19.4	16.8		43.1	22.7	62.9	38.1
Av.	9.7	4.2		29.4	21.7	18.4	33.0	20.7		29.0	17.7	33.0	22.5	7.9	3.9		35.0	27.2	24.7	23.3	16.3		47.3	27.7	65.2	46.8
Amount of decrease			17.6			29.8			29.0		17.7		31.3			13.1			22.9			32.7		31.0		50.4
% of decrease			98			65			83		53		62			97			50			88		58		57

the conditions then obtaining.[1] Under the new conditions, where fixation is eliminated, his nystagmus time was 18.8 sec. The same is true of Subj. *D.* These facts suggest that essentially different conditions obtain during the wearing of lenses, and also that the effect of lenses is to increase very markedly the time of nystagmus and the number of movements. It is not at all improbable that 'normal' nystagmus movements are gotten only when fixation is eliminated; or, in other words, any kind of fixation is possible only when the ocular muscles are being innervated from other sources than the vestibular tracts. Vestibular excitations are superimposed, therefore, upon excitations already taking place when a subject is fixating any point whatsoever.

Further examination of Table V. makes it evident that the factors which we found to be operative in the decrease under usual conditions also appear here; *viz.*, a large part of the decrease takes place at first, and there are wide individual differences in the rate of decrease and in initial values.[2] These facts, especially the observation that decrease occurs with practice, even though fixation is eliminated, are again of significance in view of the contention of certain otologists that the small decrease they found was due to 'gaze-fixing' and not to practice.[3]

c. The effect of lateral fixation. Attention has frequently been called to the fact that if, during rotation, one looks laterally in the direction of rotation the time of nystagmus is greatly increased while to look in the direction opposite to rotation causes a decrease in the time of nystagmus.[4] That is to say, the time of nystagmus is increased if the subject looks in the direction of the quick component of the ocular movements. This is also true during the after-nystagmus. For example, Subjs. *J*, *W* and *D* were rotated and so stopped

[1] See Griffith, C. R., this JOURNAL, p. 25, Table I.

[2] Griffith, C. R., this JOURNAL, pp. 27.

[3] See Fisher and Babcock, *J. Amer. Med. Ass.*, 1918, 72, 780.

[4] Fridenberg, P., 'The Non-acoustic Functions of the Labyrinth,' *Trans. Amer. Otol. Soc.*, 1908, 11, 181–182; Bárány, R., 'Untersuchungen über den vom Vestibularapparates des Ohres reflektorisch ausgelösten rhythmischen Nystagmus und seine Begleiterscheinungen,' *Monatschr. f. Ohrenhk.*, 1906, 40, 193–297.

TABLE VI

DECREASE IN TIME OF NYSTAGMUS AND NO. OF MOVEMENTS FOR SUBJECTS *W* AND *D* UNDER LATERAL FIXATION. AVERAGES OF 10 TRIALS (5*R* AND 5*L*) OF 10 TURNS EACH.

Trials	Time of Nystagmus										No. of Movements									
	Subjects										Subjects									
	W	*W* Con.	*W* Con.	*W* Con.	*W* Con.	*W* Con.	*D*	*D* Con.	*D* Con.	*D* Con.	*W*	*W* Con.	*W* Con.	*W* Con.	*W* Con.	*W* Con.	*D*	*D* Con.	*D* Con.	*D* Con.
1	28.0	13.2	9.7	11.3	8.2	5.7	25.9	17.4	11.6	7.7	71.1	29.4	21.0	27.0	16.6	9.3	46.5	33.8	19.6	7.6
2	28.7	14.5	12.4	11.2	7.0	5.9	24.7	13.6	12.0	8.8	74.6	33.7	27.4	23.4	14.6	10.8	44.8	38.5	20.0	9.5
3	27.0	15.2	13.2	9.7	6.7	4.9	24.3	16.9	14.1	6.1	72.0	32.1	26.4	24.9	13.8	10.1	43.8	35.3	23.2	6.8
4	26.8	15.6	12.4	10.7	5.9	4.8	23.8	17.3	12.5	7.1	52.1	33.4	23.7	25.2	11.7	9.1	44.0	39.7	25.5	7.7
5	24.0	13.7	11.4	11.3	5.9	4.1	20.1	12.0	11.7	6.2	50.7	28.9	23.6	26.6	13.3	7.6	41.2	23.2	19.3	6.1
Av.	26.4	14.4	11.8	10.8	6.7	5.0	23.7	15.4	12.4	7.2	64.1	31.5	24.3	27.5	14.0	9.4	44.0	34.1	21.5	7.5
6	21.0	15.8	11.2	10.8	6.3		22.4	13.4	10.3	5.8	43.1	31.8	21.7	20.9	14.7		37.9	26.7	16.3	5.6
7	17.8	15.6	12.3	8.5	6.1		23.7	13.7	9.2	2.9	40.1	33.6	24.2	18.5	12.2		51.2	23.9	12.3	2.3
8	16.1	11.7	12.4	9.2	5.1		20.2	13.0	10.1	1.0	40.6	24.8	27.1	21.5	9.6		44.5	22.8	12.2	0.9
9	15.7	13.5	13.2	9.6	6.3		13.7	12.2	7.7	0.0	37.1	28.6	23.0	21.4	12.3		32.9	21.6	10.2	0.0
10	12.4	13.5	10.8	8.2	5.2		17.3	11.6	8.6	0.0	27.3	29.9	25.8	17.5	11.1		35.0	20.1	10.0	0.0
Av.	16.7	14.0	11.9	9.3	5.8		19.4	12.8	9.2	1.9	37.6	29.7	25.4	19.9	12.0		40.3	23.0	12.2	1.8
Amount of decrease						23.9				25.9						63.5				46.5
% of decrease						85				100						89				100

that it would be necessary for them to fixate the prescribed mark out of the corner of the eye. After being rotated to their left they were instructed to look laterally, during the period of the after-nystagmus, to the right and when rotated to the right they looked, during the after-nystagmus, to the left. In other words, the gaze was directed with the quick component of the ocular movements. Under these conditions, it was found that the time of nystagmus was increased and that the number and speed of movements were enormously increased although the amplitude of the movements was smaller than during direct fixation. Table VI. shows the results from these three subjects. The average initial time is 26.7—a value 8.7 sec. larger than the corresponding value for normal conditions. Still more important is the fact that decrease in all values takes place as a result of practice in the same general manner as was found to be the case above. Bárány has declared that unless the nystagmus is intense it may be entirely concealed by looking straight ahead.[2] That the decrease in nystagmus found above is not, therefore, a result of a temporary concealment due to looking directly ahead but that it is a genuine decrease which takes place even though the conditions of fixation are such as to bring about almost a maximal effect (we have already pointed out that a maximal effect is produced when fixation is entirely eliminated) is indicated by the decrease found during lateral fixation. It was true in these cases that after-nystagmus could be almost if not quite eliminated by looking laterally in the opposite direction; but each of our subjects reported that, at first, the eye seemed to be under strain; later in the series when the intensity of nystagmus had decreased, the strain entirely disappeared.

IV. Conclusion

We come now to a concluding statement of the facts issuing from our experiments and from our introspective analyses. We have found the experience of dizziness or vertigo to be made up of a large number of processes the most

[2] Bárány, R., *op. cit.*, pp. 211, 217, 225.

prominent of which are (1) kinæsthesis from the eyes and neck and in the arms, (2) pressure from the region of the abdominal viscera, the chest and head, and (3) certain vascular processes which supply an obscure background and which give to the whole experience a characteristic shading. We have found, moreover, that the whole experience of dizziness becomes less complex and less intensive under periodic repetition. Nowhere have we been able to discover a process which could be called a 'sensation of rotation' or a 'sensation of movement.' As a result of our analysis, we are warranted in speaking only of the *perception* of movement. Furthermore, we are justified not only in saying that all of the mental processes resulting from rotation may be modified by a variety of physical and physiological factors, but also that certain mental factors modify all of the results of rotation, both organic and mental. That is to say, the organic and mental effects of rotation are an integrated group of events highly sensitive in their appearance and duration to the direction of attention, psychophysical determination, the character of the visual field, and the mode of ocular fixation.

INTERDEPENDENCE OF JUDGMENTS WITHIN THE SERIES FOR THE METHOD OF CONSTANT STIMULI

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The present experiment was devised to ascertain whether there was operative, for the method of constant stimuli, any effect of one judgment upon another within the series; that is to say, whether the passing of a judgment of a certain category has any influence upon the succeeding judgment. In other words, we wish to ascertain if the order of the presentation of the series of stimuli has any effect upon the relative frequencies of the categories of judgment.

In order to determine this matter experimentally, lifted weights were used as stimuli. The stimuli were small brass cylinders—1 inch in height and $2\frac{1}{2}$ inches in diameter—loaded to the proper weight with solder. No stimulus varied as much as 10 milligrams during the experimentation. These weights were placed about the circumference of a table with a revolving top, so that the stimuli could be brought successively directly under the hand of the observer. Thus the space errors were eliminated. The time errors were present in the first order, *i.e.*, the standard stimulus was always lifted first. The rate of lifting was regulated by the beats of a metronome so that the time errors remained constant throughout the experiment. Immediately after the lifting of each comparison weight, a judgment was given verbally in terms of the second weight. The subject employed the three categories of 'lighter,' 'equal' and 'heavier,' which were defined in the usual manner. The observed relative frequencies of the different categories of judgment upon each of the comparison pairs thus obtained were treated in accordance with the methods of calculation developed by Urban.[1]

[1] For a more detailed description of the stimuli, the method of lifting and the treatment of the results, *cf.* S. W. Fernberger, 'On the Relation of the Methods of

The series of stimuli consisted of five pairs of weights. The standard stimuli were always 100 grams and the comparison stimuli 88, 92, 96, 100 and 104 grams. Two such series were mixed together; thus the results of these series are directly comparable inasmuch as they were taken simultaneously. In the first series (known hereinafter as Series A), the 96 and 100 gram comparison pairs followed the 104 gram comparison pairs. In the second series (known hereinafter as Series B), the 96 and 100 gram comparison pairs

TABLE I

ORDER OF THE SERIES OF STIMULI

Table Numbers	I	II	III
1	104*A*	104*A*	88*A*
2	**96*A***	**96*A***	**100*B***
3	92*A*	92*A*	104*A*
4	104*B*	88*A*	**96*A***
5	**100*A***	**96*B***	92*B*
6	88*A*	92*B*	88*B*
7	**96*B***	104*B*	**96*B***
8	92*B*	**100*A***	92*A*
9	88*B*	88*B*	104*B*
10	**100*B***	**100*B***	**100*A***

immediately followed the 88 gram stimuli. Hence there were, in the one series, two stimuli of critical interest following the lightest pairs, and, in the other series, the same two stimuli following the heaviest pairs. The 96 and 100 gram comparison stimuli were chosen as the critical weights because they were most likely to fall within the interval of uncertainty and, also, because they are two of the three stimuli of the series which are likely to be most heavily weighted mathematically.[1]

The actual orders of the presentation of the stimuli employed are given in Table I. The numbers in the first column indicate the successive positions of the pairs of stimuli as

Just Perceptible Differences and Constant Stimuli,' *Psychol. Mono.*, XIV. (Whole No. 61), 1913, 6–11. S. W. Fernberger, 'On the Elimination of the Two Extreme Intensities of the Comparison Stimuli in the Method of Constant Stimuli,' *Psychol. Rev.*, XXI., 1914, 337–340. F. M. Urban, 'Hilfstabellen für die Konstanzmethode,' *Arch. f. d. ges. Psychol.*, XXIV., 1912, 236–243.

[1] *Cf.* F. M. Urban, 'Die Praxis der Konstanzmethode.' Leipzig, 1912.

arranged about the turning top table. In the other three columns are the corresponding weights of the comparison stimuli. The three columns contain the different arrangements which were given to the subjects at different times during the experimentation. The arrangements of the stimuli were changed in order to prevent the observers from learning the order of the weights. In Table I., for the sake of clearness, we have printed the numbers of the critical stimuli in heavy-faced type. The letters *A* and *B*, which follow each stimulus number, indicate the Series *A* or *B* within which the stimulus was included. It will be noted that the 92 gram weights invariably follow the 96 gram stimuli in all of the three arrangements. The heaviest and lightest pairs (88 and 104 grams) follow different stimuli in the same and in the different arrangements. These stimuli, for the most part, are judged so frequently and so easily 'lighter' and 'heavier' that it did not seem worth while to have them follow pairs of the same intensity. Such an arrangement would have necessitated the introduction of four more pairs of stimuli into the series; the data from which could not have been used. It will be remembered that each of these comparison stimuli was compared with a standard stimulus of 100 grams.

Four observers took part in this investigation. They were Lucy Day Boring, Ph.D., Edwin G. Boring, Ph.D., Carroll C. Pratt, A.M., and Matsusaburo Yokoyama, A.M.[1] These observers were untrained in the particular technique of lifting which was employed, although all of them have had considerable experience in psychological experimentation. Each subject was practiced for several hours in the manner of lifting until his hand movements became relatively automatic. After that period of practice, all of his judgments were recorded.

The instructions given the observers emphasize the fact that each comparison stimulus is to be judged solely in terms of *its own* standard stimulus which has just preceded it. The actual instructions regarding this point were as follows: "The stimuli will be presented to you in an unbroken series.

[1] This experiment was performed in the Laboratory of Experimental Psychology, Clark University, during the autumn of 1919.

You will have to divide them subjectively into pairs and then judge each second weight in terms of the first weight which has just preceded it." Naturally the repeated performance of the observers during the entire experiment assured the separation of the series of stimuli into pairs in the sense of this initial instruction. With this experimental arrangement, 500 comparison judgments were obtained on each of the ten pairs of stimuli from each of the four observers. At the end of this group of experiments, 50 more judgments, with knowledge of results, were obtained on each of the ten pairs from each of the four observers. Hence this paper has an empirical basis of 22,000 individual comparison judgments.

TABLE II. *A*

SUBJECT I., SERIES *A*

Series A	88			92			96			100			104		
	L.	E.	H.	L.	E.	H.	L.	E.	H.	L.	E.	H.	L.	E.	H.
I.....	0.84	0.14	0.02	0.68	0.26	0.06	0.36	0.40	0.24	0.32	0.30	0.38	0.00	0.08	0.92
II.....	0.78	0.18	0.04	0.62	0.32	0.06	0.44	0.26	0.30	0.30	0.28	0.42	0.08	0.22	0.70
III.....	0.88	0.12	0.00	0.56	0.30	0.14	0.54	0.24	0.22	0.20	0.38	0.42	0.06	0.24	0.70
IV.....	0.80	0.20	0.00	0.64	0.26	0.10	0.58	0.24	0.18	0.22	0.36	0.42	0.08	0.14	0.78
V.....	0.94	0.06	0.00	0.70	0.12	0.18	0.44	0.26	0.30	0.22	0.28	0.50	0.06	0.22	0.72
VI.....	0.92	0.06	0.02	0.58	0.32	0.10	0.48	0.26	0.26	0.22	0.28	0.50	0.06	0.14	0.80
VII....	0.96	0.04	0.00	0.72	0.18	0.10	0.52	0.28	0.20	0.24	0.34	0.42	0.08	0.14	0.78
VIII...	0.86	0.10	0.04	0.68	0.20	0.12	0.48	0.34	0.18	0.30	0.16	0.54	0.10	0.14	0.76
IX.....	0.96	0.04	0.00	0.72	0.20	0.08	0.44	0.32	0.24	0.20	0.32	0.48	0.04	0.14	0.82
X.....	0.82	0.18	0.00	0.60	0.30	0.10	0.46	0.32	0.22	0.26	0.22	0.42	0.04	0.14	0.82
W. K...	0.86	0.14	0.00	0.78	0.14	0.08	0.48	0.38	0.14	0.30	0.22	0.48	0.04	0.16	0.80

TABLE II. *B*

SUBJECT I., SERIES *B*

Series B	88			92			96			100			104		
	L.	E.	H.	L.	E.	H.	L.	E.	H.	L.	E.	H.	L.	E.	H.
I.....	0.68	0.26	0.06	0.58	0.26	0.16	0.22	0.34	0.44	0.10	0.20	0.70	0.06	0.06	0.88
II.....	0.74	0.22	0.04	0.62	0.30	0.08	0.36	0.26	0.38	0.08	0.28	0.64	0.06	0.22	0.72
III.....	0.84	0.12	0.04	0.58	0.32	0.10	0.36	0.30	0.34	0.06	0.26	0.68	0.06	0.20	0.74
IV.....	0.80	0.14	0.06	0.58	0.28	0.14	0.34	0.32	0.34	0.06	0.30	0.64	0.04	0.14	0.82
V.....	0.84	0.12	0.04	0.68	0.16	0.16	0.30	0.30	0.40	0.06	0.20	0.74	0.06	0.12	0.82
VI.....	0.80	0.10	0.10	0.70	0.18	0.12	0.22	0.44	0.34	0.10	0.32	0.58	0.02	0.20	0.78
VII....	0.88	0.10	0.02	0.58	0.28	0.14	0.12	0.36	0.52	0.08	0.22	0.70	0.04	0.12	0.84
VIII...	0.80	0.18	0.02	0.56	0.34	0.10	0.22	0.28	0.50	0.04	0.28	0.68	0.04	0.16	0.80
IX.....	0.86	0.10	0.04	0.56	0.34	0.10	0.16	0.34	0.50	0.08	0.30	0.62	0.02	0.04	0.94
X.....	0.78	0.20	0.02	0.64	0.26	0.10	0.18	0.20	0.62	0.12	0.24	0.64	0.04	0.08	0.88
W. K...	0.80	0.16	0.04	0.52	0.30	0.18	0.42	0.24	0.34	0.08	0.34	0.58	0.08	0.08	0.84

TABLE III. *A*

SUBJECT II., SERIES *A*

Series A	88			92			96			100			104		
	L.	E.	H.	L.	E.	H.	L.	E.	H.	L.	E.	H.	L.	E.	H.
I.....	0.78	0.18	0.04	0.52	0.24	0.24	0.32	0.36	0.32	0.26	0.30	0.44	0.04	0.10	0.86
II.....	0.76	0.16	0.08	0.50	0.24	0.26	0.36	0.30	0.34	0.18	0.24	0.58	0.04	0.06	0.90
III.....	0.76	0.16	0.08	0.50	0.30	0.20	0.24	0.38	0.38	0.16	0.20	0.64	0.04	0.06	0.90
IV.....	0.72	0.24	0.04	0.32	0.46	0.22	0.18	0.50	0.32	0.06	0.44	0.50	0.04	0.16	0.80
V.....	0.80	0.18	0.02	0.48	0.36	0.16	0.24	0.44	0.32	0.18	0.30	0.52	0.02	0.02	0.96
VI.....	0.82	0.14	0.04	0.46	0.32	0.22	0.30	0.40	0.30	0.14	0.34	0.52	0.06	0.04	0.90
VII....	0.72	0.22	0.06	0.44	0.30	0.26	0.30	0.32	0.38	0.22	0.22	0.56	0.00	0.10	0.90
VIII...	0.70	0.26	0.04	0.42	0.40	0.18	0.22	0.46	0.32	0.12	0.34	0.54	0.04	0.06	0.90
IX.....	0.70	0.28	0.02	0.36	0.40	0.24	0.42	0.30	0.28	0.16	0.38	0.46	0.02	0.12	0.86
X.....	0.66	0.28	0.06	0.36	0.40	0.24	0.34	0.36	0.30	0.14	0.36	0.50	0.00	0.08	0.92
W. K...	0.72	0.20	0.08	0.42	0.34	0.24	0.48	0.36	0.16	0.14	0.40	0.46	0.04	0.10	0.86

TABLE III. *B*

SUBJECT II., SERIES *B*

Series B	88			92			96			100			104		
	L.	E.	H.	L.	E.	H.	L.	E.	H.	L.	E.	H.	L.	E.	H.
I.....	0.76	0.22	0.02	0.46	0.38	0.16	0.22	0.30	0.48	0.06	0.14	0.80	0.00	0.16	0.84
II.....	0.76	0.12	0.12	0.54	0.26	0.20	0.16	0.26	0.58	0.12	0.08	0.80	0.04	0.06	0.90
III.....	0.64	0.28	0.08	0.44	0.38	0.18	0.16	0.26	0.58	0.14	0.18	0.68	0.04	0.10	0.86
IV.....	0.60	0.36	0.04	0.24	0.54	0.22	0.10	0.38	0.52	0.04	0.24	0.72	0.00	0.18	0.82
V.....	0.58	0.36	0.06	0.36	0.44	0.20	0.14	0.32	0.54	0.10	0.14	0.76	0.00	0.06	0.94
VI.....	0.62	0.28	0.10	0.26	0.44	0.30	0.08	0.32	0.60	0.04	0.18	0.78	0.00	0.06	0.94
VII....	0.58	0.28	0.14	0.36	0.34	0.30	0.12	0.20	0.68	0.08	0.08	0.84	0.00	0.08	0.92
VIII...	0.62	0.32	0.06	0.34	0.48	0.18	0.10	0.22	0.68	0.04	0.18	0.78	0.00	0.06	0.94
IX.....	0.60	0.30	0.10	0.28	0.52	0.20	0.08	0.26	0.66	0.04	0.22	0.74	0.00	0.08	0.92
X.....	0.62	0.28	0.10	0.38	0.32	0.30	0.10	0.24	0.66	0.02	0.24	0.74	0.00	0.08	0.92
W. K...	0.70	0.22	0.08	0.40	0.42	0.18	0.14	0.34	0.52	0.06	0.24	0.70	0.00	0.10	0.90

TABLE IV. *A*

SUBJECT III., SERIES *A*

Series	88			92			96			100			104		
	L.	E.	H.	L.	E.	H.	L.	E.	H.	L.	E.	H.	L.	E.	H.
I.....	0.80	0.12	0.08	0.60	0.18	0.22	0.36	0.26	0.38	0.32	0.26	0.42	0.06	0.16	0.78
II.....	0.84	0.08	0.08	0.66	0.08	0.26	0.40	0.22	0.38	0.38	0.20	0.42	0.06	0.14	0.80
III.....	0.88	0.06	0.06	0.68	0.14	0.18	0.50	0.24	0.26	0.30	0.26	0.44	0.08	0.14	0.78
IV.....	0.76	0.12	0.12	0.56	0.26	0.18	0.50	0.22	0.28	0.38	0.22	0.40	0.12	0.12	0.76
V.....	0.80	0.12	0.08	0.62	0.20	0.18	0.52	0.18	0.30	0.38	0.24	0.38	0.08	0.12	0.80
VI.....	0.78	0.16	0.06	0.56	0.22	0.22	0.54	0.30	0.16	0.32	0.26	0.42	0.10	0.20	0.70
VII....	0.84	0.10	0.06	0.66	0.18	0.16	0.62	0.22	0.16	0.48	0.24	0.28	0.04	0.18	0.78
VIII...	0.74	0.10	0.16	0.66	0.14	0.20	0.54	0.16	0.30	0.48	0.08	0.44	0.04	0.10	0.86
IX.....	0.86	0.08	0.06	0.54	0.22	0.24	0.54	0.18	0.28	0.38	0.22	0.40	0.02	0.10	0.88
X.....	0.78	0.14	0.08	0.70	0.20	0.10	0.58	0.26	0.16	0.32	0.26	0.42	0.04	0.06	0.90
W. K...	0.94	0.02	0.04	0.72	0.12	0.16	0.54	0.06	0.40	0.36	0.14	0.50	0.00	0.10	0.90

TABLE IV. *B*

SUBJECT III., SERIES *B*

Series	88			92			96			100			104		
	L.	E.	H.	L.	E.	H.	L.	E.	H.	L.	E.	H.	L.	E.	H.
I.....	0.66	0.14	0.20	0.56	0.08	0.36	0.30	0.14	0.56	0.10	0.24	0.66	0.10	0.08	0.82
II.....	0.72	0.18	0.10	0.44	0.28	0.28	0.26	0.18	0.56	0.08	0.18	0.74	0.04	0.16	0.80
III.....	0.84	0.08	0.08	0.66	0.12	0.22	0.32	0.12	0.56	0.12	0.12	0.76	0.08	0.12	0.80
IV.....	0.70	0.18	0.12	0.54	0.20	0.26	0.22	0.16	0.62	0.08	0.14	0.78	0.10	0.16	0.74
V.....	0.78	0.16	0.06	0.62	0.14	0.24	0.24	0.08	0.68	0.14	0.14	0.72	0.08	0.12	0.80
VI.....	0.70	0.18	0.12	0.46	0.34	0.20	0.22	0.14	0.64	0.12	0.16	0.72	0.04	0.26	0.70
VII....	0.80	0.14	0.06	0.68	0.16	0.16	0.26	0.22	0.52	0.16	0.14	0.70	0.08	0.14	0.78
VIII...	0.84	0.04	0.12	0.74	0.12	0.14	0.20	0.16	0.64	0.06	0.12	0.82	0.06	0.14	0.80
IX.....	0.78	0.16	0.06	0.58	0.24	0.18	0.24	0.22	0.54	0.08	0.18	0.74	0.02	0.06	0.92
X.....	0.78	0.18	0.04	0.72	0.20	0.08	0.22	0.16	0.62	0.08	0.18	0.74	0.06	0.04	0.90
W. K...	0.94	0.04	0.02	0.72	0.12	0.16	0.20	0.14	0.66	0.10	0.10	0.80	0.00	0.10	0.90

TABLE V. *A*

SUBJECT IV, SERIES *A*

Series	88			92			96			100			104		
	L.	E.	H.	L.	E.	H.	L.	E.	H.	L.	E.	H.	L.	E.	H.
I.....	0.80	0.18	0.02	0.46	0.34	0.20	0.36	0.26	0.38	0.22	0.26	0.52	0.08	0.12	0.80
II.....	0.80	0.14	0.06	0.54	0.22	0.24	0.38	0.24	0.38	0.24	0.26	0.50	0.04	0.18	0.78
III.....	0.76	0.14	0.10	0.60	0.18	0.22	0.44	0.30	0.26	0.24	0.28	0.48	0.00	0.18	0.82
IV.....	0.64	0.26	0.10	0.42	0.34	0.24	0.34	0.40	0.26	0.24	0.28	0.48	0.00	0.18	0.82
V.....	0.70	0.20	0.10	0.54	0.34	0.12	0.40	0.36	0.24	0.30	0.34	0.36	0.00	0.08	0.92
VI.....	0.66	0.22	0.12	0.54	0.20	0.16	0.46	0.28	0.28	0.32	0.28	0.40	0.00	0.08	0.92
VII....	0.76	0.16	0.08	0.56	0.24	0.20	0.48	0.18	0.34	0.28	0.30	0.42	0.00	0.12	0.88
VIII...	0.68	0.22	0.10	0.58	0.18	0.24	0.44	0.20	0.36	0.34	0.14	0.52	0.02	0.14	0.84
IX.....	0.80	0.14	0.06	0.54	0.32	0.14	0.46	0.28	0.26	0.30	0.20	0.50	0.00	0.10	0.90
X.....	0.86	0.10	0.04	0.60	0.26	0.14	0.50	0.18	0.32	0.28	0.24	0.48	0.00	0.10	0.90
W. K...	0.70	0.24	0.06	0.60	0.30	0.10	0.48	0.16	0.36	0.28	0.22	0.50	0.00	0.10	0.90

TABLE V. *B*

SUBJECT IV., SERIES *B*

Series	88			92			96			100			104		
	L.	E.	H.	L.	E.	H.	L.	E.	H.	L.	E.	H.	L.	E.	H.
I.....	0.62	0.26	0.12	0.36	0.36	0.28	0.16	0.24	0.60	0.06	0.26	0.68	0.00	0.14	0.86
II.....	0.64	0.26	0.10	0.40	0.32	0.28	0.16	0.18	0.66	0.04	0.22	0.74	0.02	0.08	0.90
III.....	0.68	0.16	0.16	0.38	0.30	0.32	0.08	0.28	0.64	0.06	0.20	0.74	0.02	0.08	0.90
IV.....	0.66	0.26	0.08	0.38	0.36	0.26	0.06	0.32	0.62	0.06	0.16	0.78	0.00	0.14	0.86
V.....	0.74	0.22	0.04	0.40	0.44	0.16	0.10	0.24	0.66	0.06	0.20	0.74	0.00	0.10	0.90
VI.....	0.74	0.18	0.08	0.52	0.30	0.18	0.10	0.28	0.62	0.06	0.18	0.76	0.00	0.08	0.92
VII....	0.76	0.10	0.14	0.56	0.34	0.10	0.12	0.24	0.64	0.08	0.18	0.74	0.00	0.12	0.88
VIII...	0.76	0.16	0.08	0.48	0.30	0.22	0.14	0.20	0.66	0.06	0.12	0.82	0.02	0.12	0.86
IX.....	0.78	0.16	0.06	0.52	0.34	0.14	0.10	0.24	0.66	0.04	0.14	0.82	0.00	0.16	0.84
X.....	0.80	0.20	0.00	0.56	0.34	0.10	0.12	0.30	0.58	0.02	0.24	0.74	0.00	0.14	0.86
W. K...	0.66	0.26	0.08	0.62	0.34	0.04	0.14	0.28	0.58	0.04	0.18	0.78	0.00	0.10	0.90

The observed relative frequencies of the different categories of judgment are to be found in Tables II. to V. inclusive. Two tables are given to the results of each subject,—one table for Series *A* and the other for Series *B*. Hence Tables II. *A* and II. *B* contain the results of Series *A* and Series *B* respectively for Subject I.; Tables III. *A* and III. *B* for Subject II., and so forth. These tables are all similar in form. In the first column are numbers designating the fractions of 50 judgments on each comparison pair in the order in which they were taken. These are the fractions into which we divided the results for purposes of calculation. In the next three columns are the observed relative frequencies of judgment obtained on the 88 gram stimuli. In the next three columns are the observed relative frequencies of judgment on the 92 gram stimuli, and so on. In the bottom row of each table, marked W. K., are the observed relative frequencies of judgment of the series with knowledge taken from each observer at the end of the experiments.

The observed relative frequencies in each column, although not identical, are still very similar. When one compares the similar columns for the two series for the same subject, however, certain regular discrepancies are apparent. It will be found that the values for the three categories of judgment for the 88, 92 and 104 gram stimuli show a very close approximation when one compares the values in the two tables for the same subject. Likewise, the observed relative frequencies for the equality judgments of the 96 and 100 gram weights are similar. It will, however, be noted, in every case, that the values of the observed relative frequencies of the 'lighter' judgments for the 96 and 100 gram stimuli are larger in Series *A* than in Series *B*. Similarly, in every case, the values of the observed relative frequencies of the 'heavier' judgments for the 96 and 100 gram stimuli are smaller in Series *A* than in Series *B*. It will be remembered that the 96 and 100 gram stimuli were the critical weights.

From this it is evident that the order of the presentation of the series of stimuli has a marked effect upon the relative frequencies of the categories of judgment. When pairs which

contain the 96 and 100 gram weights followed those which contained the 104 gram stimuli (Series *A*) there was a strong tendency for them to be judged 'lighter.' When they followed the 88 gram stimuli (Series *B*), there was an equally strong tendency for them to be judged 'heavier.'

These relations are more obviously apparent when we throw the observed relative frequencies into the form of the curves of the psychometric functions. These curves are represented in Figures 1–4. Each figure represents the results

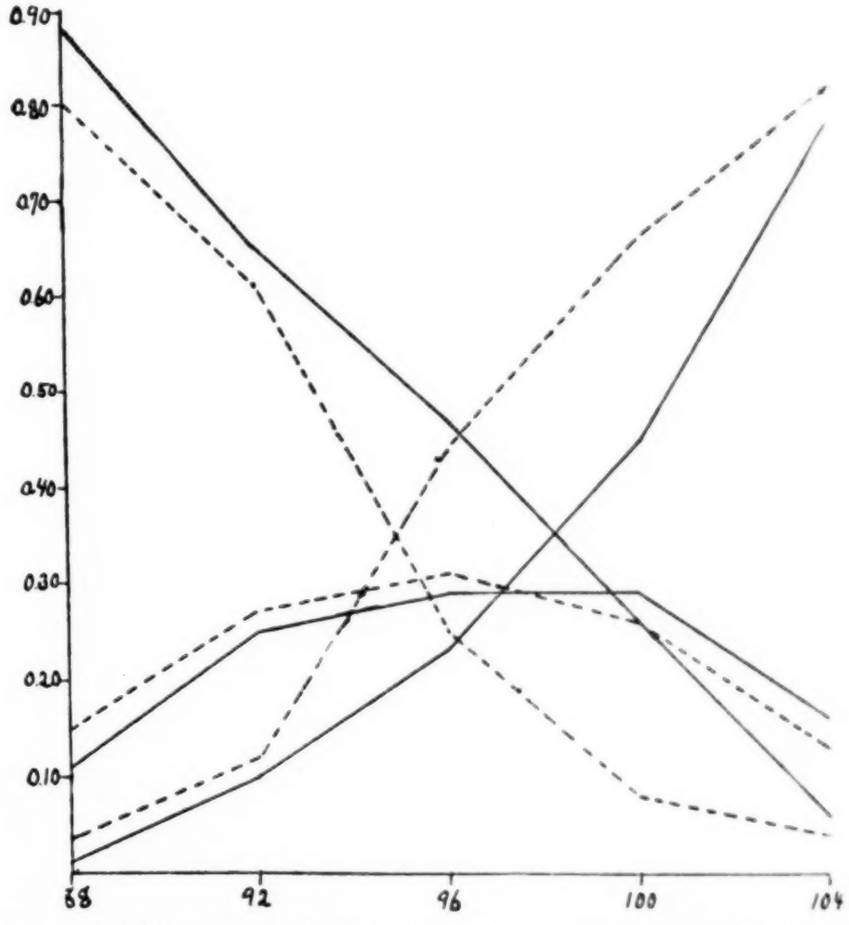

FIG. 1. Solid line represents Series *A*. (96 and 100 gram stimuli following 104.) Broken line represents Series *B*. (96 and 100 gram stimuli following 88.)

of one observer. The curves for Series *A* and Series *B* are superimposed upon one another. The charts are constructed in similar fashion. The intensities of the comparison stimuli are laid off along the abscissa, while the observed relative frequencies are erected as ordinates. These curves of the

psychometric functions represent the averages of the values found in the first ten rows of Tables II.-V.

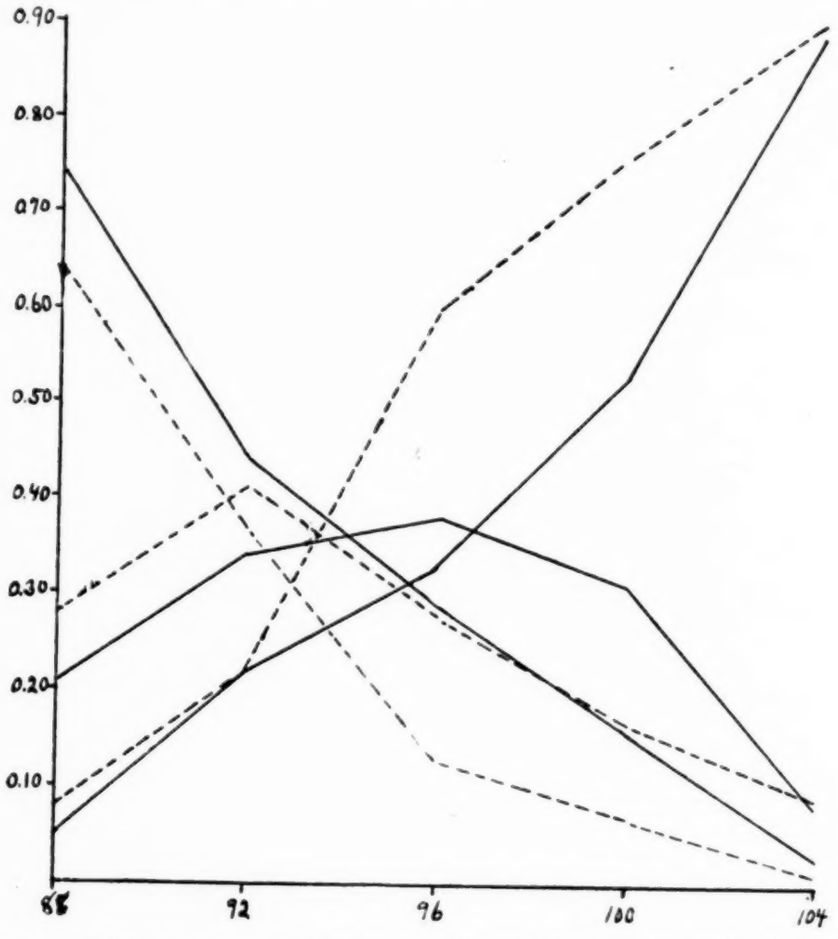

FIG. 2. Solid line represents Series *A*. (96 and 100 gram stimuli following 104.) Broken line represents Series *B*. (96 and 100 gram stimuli following 88.)

In a consideration of these curves several relations become apparent. It will be observed that the curves of the psychometric functions of the equality judgments are relatively similar in form for the two series. This relation is particularly true for Subjects III. and IV. (Figs. 3 and 4) and especially true for Subject I. (Fig. 1). In considering the curves of the psychometric functions of the 'lighter' judgments, it will be noted, for all of the subjects, that the curves of the two series are relatively coincident for the lesser intensities of stimulus. Then the curves draw apart for the values of 96 and 100 grams—the critical stimuli—and finally the curves assume very similar values for the 104 gram weights. The

curves of the psychometric functions of the 'heavier' judgments have similar form relations to those for the 'lighter' judgments, except that the spatial relations are reversed. Hence, at those points where the two curves separate for the two critical stimuli, in the case of the 'lighter' judgments the curve for Series *A* (96 and 100 following 104) has values greater than those for Series *B* (96 and 100 following 88); whereas in the case of the 'heavier' judgments, the values for Series *A* are less than those for Series *B*.

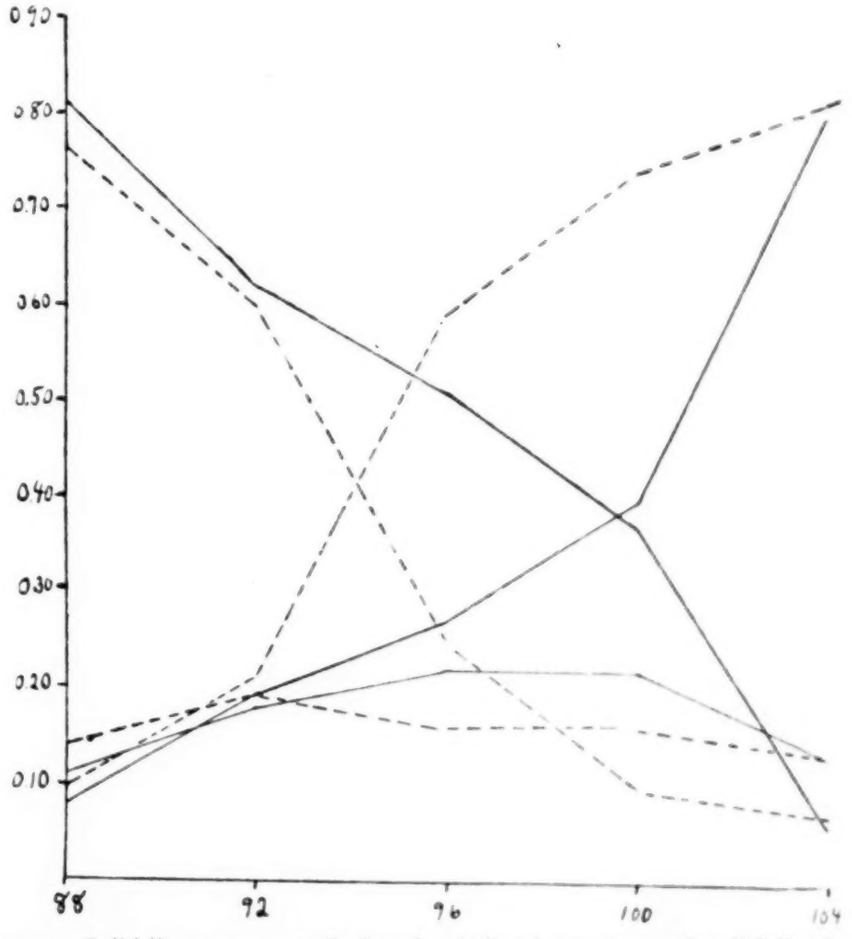

FIG. 3. Solid line represents Series *A*. (96 and 100 gram stimuli following 104.) Broken line represents Series *B*. (96 and 100 gram stimuli following 88.)

It is of interest to study the effects of these differences of the observed relative frequencies for the two series upon the constants of the psychometric functions, upon the values of the thresholds, and upon the values of the points of subjective equality and of the intervals of uncertainty. These values

are given in Tables VI.–IX. Two tables are devoted to the values of each subject. The values for Series *A* of Subject I. are to be found in Table VI. *A* and the values for Series *B* for the same subject are to be found in Table VI. *B*. The tables are similar in form. In the first columns are recorded

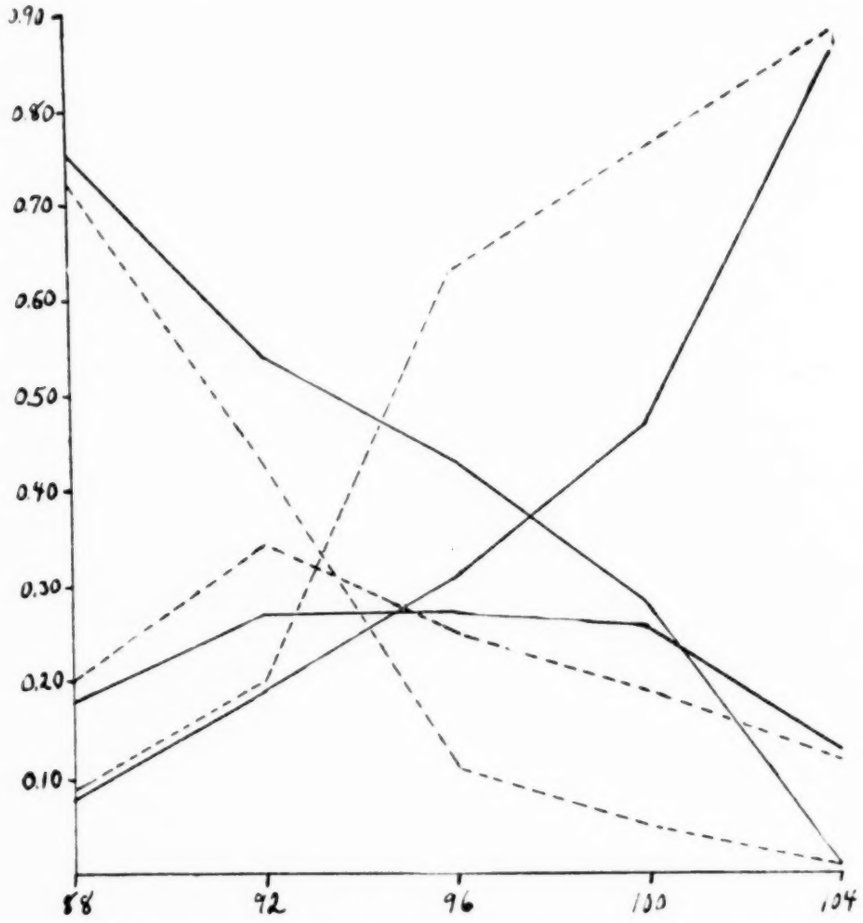

FIG. 4. Solid line represents Series *A*. (96 and 100 gram stimuli following 104.) Broken line represents Series *B*. (96 and 100 gram stimuli following 88.)

the numbers of the fractions of 50 judgments on each comparison pair into which the complete data were divided. In the next two columns are the values of the coefficients of precision of the curves of the psychometric functions of the 'lighter' and 'heavier' judgments respectively (h_1 and h_2). In the next two columns are the values of the lower and upper thresholds respectively (S_1 and S_2); and in the next column the values of the interval of uncertainty ($S_2 - S_1$). In the last columns are the values of the point of subjective equality

TABLE VI. *A*

SUBJECT I., SERIES *A*

Fractions	h_1	h_2	S_1	S_2	Interval of Uncertainty	Point of Subjective Equality
I.....	0.0914	0.1450	95.16	99.70	4.54	97.94
II.....	0.0876	0.1041	94.58	100.67	6.09	97.88
III.....	0.1092	0.0970	94.80	100.86	6.06	97.65
IV.....	0.0967	0.1215	95.07	100.47	5.40	98.08
V.....	0.1292	0.0891	95.42	99.73	4.31	97.18
VI.....	0.1154	0.1247	94.92	99.59	4.67	97.35
VII.....	0.1283	0.1223	96.12	100.36	4.24	98.19
VIII.....	0.0994	0.1138	95.61	99.87	4.26	97.89
IX.....	0.1562	0.1351	95.06	99.71	4.65	97.21
X.....	0.0905	0.1277	95.00	100.01	5.01	97.93
Ave.....			95.17	100.10	4.92	97.73
P. E.....			0.094	0.096	0.147	0.078
W. K.....	0.1145	0.1413	95.80	100.19	4.39	98.23

TABLE VI. *B*

SUBJECT I, SERIES *B*

Fractions	h_1	h_2	S_1	S_2	Interval of Uncertainty	Point of Subjective Equality
I.....	0.1005	0.1244	91.86	97.13	5.27	94.77
II.....	0.1077	0.1105	93.05	98.97	5.92	96.05
III.....	0.1215	0.1145	93.45	98.76	5.31	96.03
IV.....	0.1231	0.1147	93.05	98.22	5.17	95.54
V.....	0.1285	0.1217	93.68	97.54	3.86	95.56
VI.....	0.1365	0.0985	93.16	98.73	5.57	95.50
VII.....	0.1124	0.1258	95.36	97.27	1.91	96.37
VIII.....	0.1297	0.1228	92.43	97.80	5.37	95.04
IX.....	0.1467	0.1410	92.64	97.22	4.58	94.90
X.....	0.1211	0.1319	92.78	97.09	4.31	95.02
Ave.....			93.15	97.87	4.73	95.48
P. E.....			0.199	0.161	0.547	0.118
W. K.....	0.1040	0.1116	93.27	98.44	5.17	95.94

$\left(\frac{c_1 + c_2}{h_1 + h_2}\right)$. At the bottom of the columns for the thresholds, the intervals of uncertainty and the points of subjective equality are the averages for these values within the column, with the probable errors of these averages. Finally in the laws rows—marked W. K.,—are given the values for the series with knowledge obtained after the other data had been collected.

When one compares the values for the coefficients of precision of the psychometric functions for the 'lighter' judgments (h_1), one finds that the values for Series *A* are almost invariably smaller than the corresponding values for Series *B*. This relationship holds for almost every group with every subject. It means that the curves of the 'lighter' judgments descend more steeply for the heavily weighted

TABLE VII. *A*

SUBJECT II., SERIES *A*

Fractions	h_1	h_2	S_1	S_2	Interval of Uncertainty	Point of Subjective Equality
I.....	0.0906	0.1018	93.22	98.79	5.57	96.17
II.....	0.0985	0.1057	92.82	97.54	4.72	95.26
III.....	0.1044	0.1244	92.26	97.64	5.38	95.19
IV.....	0.1083	0.0999	90.43	98.93	8.50	94.51
V.....	0.1105	0.1398	92.47	98.31	5.84	95.73
VI.....	0.1056	0.1149	92.67	98.28	5.61	95.59
VII.....	0.0787	0.1080	91.99	97.54	5.55	95.20
VIII.....	0.0994	0.1212	91.11	98.25	7.14	95.03
IX.....	0.0871	0.1016	91.56	99.14	7.58	95.65
X.....	0.0793	0.1083	90.81	98.19	7.38	95.07
Ave.....			91.93	98.26	6.33	95.34
P. E.....			0.198	0.122	0.293	0.098
W. K.....	0.0905	0.0965	92.46	99.27	6.81	95.97

TABLE VII. *B*

SUBJECT II., SERIES *B*

Fractions	h_1	h_2	S_1	S_2	Interval of Uncertainty	Point of Subjective Equality
I.....	0.1315	0.1305	91.68	96.96	5.28	94.31
II.....	0.1143	0.1189	92.07	95.57	3.50	93.85
III.....	0.0923	0.1117	90.48	96.58	6.10	93.82
IV.....	0.1234	0.1094	88.88	96.92	8.04	92.66
V.....	0.0938	0.1369	89.25	96.01	6.76	93.26
VI.....	0.1308	0.1224	89.15	95.06	5.91	92.00
VII.....	0.1017	0.1175	89.28	94.40	5.12	92.03
VIII.....	0.1282	0.1407	89.63	95.50	5.87	92.70
IX.....	0.1273	0.1217	89.07	96.39	7.32	92.65
X.....	0.1388	0.1146	89.87	95.08	5.21	92.22
Ave.....			89.94	95.85	5.91	92.95
P. E.....			0.240	0.183	0.597	0.384
W. K.....	0.1286	0.1207	90.67	96.53	5.86	93.51

TABLE VIII. *A*

SUBJECT III., SERIES *A*

Fractions	h_1	h_2	S_1	S_2	Interval of Uncertainty	Point of Subjective Equality
I.....	0.0912	0.0850	94.25	99.23	4.98	96.65
II.....	0.0945	0.0837	95.26	98.92	3.66	96.99
III.....	0.1057	0.0962	95.72	99.80	4.08	97.66
IV.....	0.0708	0.0794	94.96	100.13	5.17	97.70
V.....	0.0831	0.0903	95.50	99.83	4.33	97.75
VI.....	0.0777	0.0836	96.02	100.96	4.94	98.58
VII.....	0.0840	0.0929	96.97	101.30	4.33	99.25
VIII.....	0.0695	0.0822	96.11	98.83	2.72	97.59
IX.....	0.0914	0.0981	94.56	99.04	4.51	96.90
X.....	0.0921	0.1126	95.64	99.74	4.10	97.89
Ave.....			95.50	99.78	4.28	97.70
P. E.....			0.169	0.179	0.154	0.166
W. K.....	0.1024	0.1195	97.01	98.20	1.19	97.65

TABLE VIII. *B*

SUBJECT III., SERIES *B*

Fractions	h_1	h_2	S_1	S_2	Interval of Uncertainty	Point of Subjective Equality
I.....	0.0858	0.0754	91.94	94.72	2.78	93.24
II.....	0.1068	0.0951	91.47	96.17	4.70	93.68
III.....	0.1159	0.1038	93.92	96.47	2.55	95.13
IV.....	0.0926	0.0870	91.76	96.04	4.28	93.83
V.....	0.0910	0.1010	92.79	96.12	3.33	94.54
VI.....	0.1011	0.0825	91.35	96.74	5.39	93.77
VII.....	0.1085	0.1063	93.66	97.36	3.70	95.49
VIII.....	0.1346	0.1053	93.56	95.97	2.41	94.62
IX.....	0.1300	0.1324	92.52	96.28	3.76	94.42
X.....	0.1212	0.1409	93.17	96.50	3.33	94.96
Ave.....			92.61	96.24	3.62	94.37
P. E.....			0.203	0.143	0.204	0.154
W K.....	0.1783	0.1409	93.92	95.95	2.03	94.82

values for Series *B*. Similar relations hold for the coefficients of precision of the psychometric functions of the 'heavier' judgments (h_2). Here the coefficients of precision for Series *A* are usually smaller than the corresponding values for Series *B*. An inspection of the curves of the psychometric functions of the two series (Figs. I–IV.) shows the relation. It will be remembered that the different observed relative frequencies

of judgment are differently weighted mathematically; therefore the values of h_1 and h_2 indicate the steepness of the curves of the psychometric functions of the 'lighter' and 'heavier' judgments predominately for the central values.

TABLE IX. *A*

SUBJECT IV, SERIES *A*

Fractions	h_1	h_2	S_1	S_2	Interval of Uncertainty	Point of Subjective Equality
I	0.0897	0.1046	93.08	98.66	5.58	96.09
II	0.0984	0.0865	93.63	97.57	3.94	95.50
III	0.0817	0.0904	94.24	99.07	4.83	96.81
IV	0.0606	0.0887	91.15	99.00	7.85	95.82
V	0.0618	0.1006	93.54	99.70	6.16	97.36
VI	0.0500	0.0948	93.82	99.10	5.28	97.28
VII	0.0713	0.0984	94.54	98.78	4.24	97.00
VIII	0.0710	0.0924	93.77	98.16	4.39	96.25
IX	0.0744	0.1185	94.66	98.78	4.12	97.19
X	0.0902	0.1225	95.38	98.69	3.31	97.29
Ave.			93.78	98.75	4.97	96.66
P. E.			0.242	0.123	0.2982	0.1459
W. K.	0.0638	0.1214	94.44	98.52	4.08	97.11

TABLE IX. *B*

SUBJECT IV., SERIES *B*

Fractions	h_1	h_2	S_1	S_2	Interval of Uncertainty	Point of Subjective Equality
I	0.1114	0.0976	89.95	95.84	5.89	92.70
II	0.1158	0.1120	90.20	95.26	5.06	92.69
III	0.1210	0.1003	90.20	94.80	4.60	92.28
IV	0.1298	0.1118	90.08	95.64	5.56	92.65
V	0.1402	0.1269	90.86	96.08	5.22	93.34
VI	0.1432	0.1293	91.43	95.78	4.35	93.50
VII	0.1385	0.1121	91.88	95.88	4.00	93.67
VIII	0.1325	0.1175	91.51	95.52	4.01	93.39
IX	0.1622	0.1228	91.61	96.06	4.45	93.53
X	0.1763	0.1264	91.86	96.86	4.86	93.89
Ave.			90.96	95.77	4.80	93.16
P. E.			0.168	0.116	0.1357	0.1146
W. K.	0.1330	0.1339	91.42	96.30	4.88	93.86

When one compares the values of both the lower and upper thresholds (S_1 and S_2) for Series *A* and *B*, a very striking difference is apparent. In both cases the values for Series *B*

are less than those for Series *A*. This relation is true for every fraction and for every subject (*cf.* Tables VI.–IX.). In the case of the lower threshold, the average difference for all four observers is 2.43 grams. The averages for each individual observer are: for Subject I., 2.02 grams; Subject II., 1.99 grams; Subject III., 2.89 grams and for Subject IV., 2.82 grams. The total average difference for all four observers, in the case of the upper threshold, is 2.79 grams. The averages for each individual observer are: for Subject I., 2.23 grams; Subject II., 2.41 grams; Subject III., 3.54 grams and for Subject IV., 2.98 grams. It will be noted that the magnitudes of the differences in the case of the upper and lower thresholds are similar but by no means identical. In the case of every subject, the amount of shift of the upper threshold along the abscissa is greater than that for the lower threshold. Hence the effect of the changed order of the series seems to exert a slightly greater influence upon the upper limen. Absolutely, these differences in the position of the upper and lower threshold values on the abscissa are very large.

The average values of the intervals of uncertainty and of the points of subjective equality for all four subjects are brought together in Table X. In the second and third

TABLE X

Subjects	Average Interval of Uncertainty		Average Points of Subjective Equality		Differences Average Intervals of Uncertainty	Index of Significance of Average Intervals of Uncertainty	Differences Average Points of Subjective Equality	Index of Significance of Average Points of Subjective Equality
	Series *A*	Series *B*	Series *A*	Series *B*				
I.....	4.92	4.73	97.73	95.48	0.19	0.1233	2.25	0.9999
II.....	6.33	5.91	95.34	92.95	0.42	0.2335	2.39	0.9798
III....	4.28	3.62	97.70	94.37	0.66	0.5419	3.33	0.9999
IV.....	4.97	4.80	96.66	93.16	0.17	0.2736	3.50	1.0000
Ave....					0.36	0.2931	2.87	0.9949

columns are the average values of the intervals of uncertainty for Series *A* and *B* respectively; in the next two columns, the average values of the points of subjective equality for Series *A* and *B* respectively, and, in the next column, the average differences between the two intervals of uncertainty

found in the second and third columns of the same table. In the next column are the values of the probability that the difference will not vary by an amount greater than itself, which is an index of the significance of the difference.[1] In the last two columns are found the differences in the average points of subjective equality, and the indices of significance of these differences. In the bottom line are the average differences between the intervals of uncertainty and between the points of subjective equality, together with the average values of the indices of significance in each case.

The averages of the size of the interval of uncertainty are fairly similar. The general average for all four subjects gives a value of 0.36 grams with an average index of significance of only 0.2931. The largest difference for any observer is that for Subject III., with a value of 0.66 grams and an index of significance of 0.5419. These differences are relatively small and the indices of significance are relatively low. One is tempted to remark that they are due to chance errors and may, therefore, be disregarded. A closer examination of the facts, however, leads to a different conclusion.

It will be noted that the average differences between the intervals of uncertainty for Series *A* and *B* are in the same direction and bear the same sign for all four subjects. In every case the interval of uncertainty for Series *B* is smaller than the corresponding value for Series *A*. It is improbable that this uniformity is merely a chance result, since it holds in 70 *per cent.* of the fractions into which the complete data were divided. For Subject I., the results of 7 fractions agree with the sign of the general average, with 3 fractions opposed. The figures for the other observers are: Subject II., 7 fractions agree with 3 opposed; Subject III., 8 fractions agree with 2 opposed; Subject IV., 6 fractions agree with 4 opposed.

[1] These values are calculated in the following manner. The probable error of the difference is first obtained by the formula $PE_D = \sqrt{PE_A^2 + PE_B^2}$. One then determines a value (z) which is the ratio between the difference and the probable error of the difference and which is defined by the formula $z = D/PE_D$. One then determines the value of P_z or the probability that the difference will not vary by an amount greater than itself in a special table of the probability integral. William W. Johnson, 'The Theory of Errors and Method of Least Squares,' New York, 1892, Table II., p. 154.

An explanation of this state of affairs is to be found in a further consideration of the curves of the psychometric functions. It will be remembered that the values of the coefficients of precision of the curves of the psychometric functions of both the 'lighter' and 'heavier' judgments are larger for Series *B* than for Series *A*. Hence these curves for Series *B* would be steeper than those for Series *A*, since the larger the value of the coefficient of precision, the steeper the curve. As a result of this increase in the steepness of the curves of the psychometric functions for the 'lighter' and 'heavier' judgments, one might expect the slight narrowing of the interval of uncertainty in Series *B* which actually does occur. This difference in the size of the intervals of uncertainty, however, although it does not seem to be due to chance results, is relatively slight and perhaps may properly be disregarded from a practical point of view.

When we consider the points of subjective equality for our two series we find an extremely significant difference in their values. In every fraction for every subject the point of subjective equality for Series *B* (96 and 100 grams after 88 grams) assumes a lower value than the corresponding one for Series *A* (96 and 100 after 104 grams). The averages of these differences of the points of subjective equality are given, for the four subjects, in next to the last column of Table X. In the last column of the same table are the indices of significance of this difference. The average difference for all four subjects is 2.87 grams, with an index of significance of 0.9949. The smallest difference for any observer (Subject I.) is 2.25 grams, but it has an index of significance of 0.9999 or mathematical certainty. The lowest index of significance is 0.9798 (Subject II.) which is, nevertheless, absolutely very high. Moreover the differences between the points of subjective equality for every fraction and for every observer have the same sign.

An inspection of Figs. 1–4 emphasizes and explains this difference. It will be remembered that the point of subjective equality (defined by the formula $\left(\frac{h_1 + h_2}{c_1 + c_2}\right)$ is the point

of intersection of the curves of the psychometric functions for the 'lighter' and 'heavier' judgments. Due to the distortion of the parts of the curves for the values of the 96 and 100 gram comparison stimuli in the two series, these curves cross at different places with regard to the abscissa. Hence it will be observed that the curves for Series *B* always cross at lower values on the abscissa than those for Series *A*.

These relations are by far the most significant that come out of the present investigation. A shift in the point of subjective equality of more than $2\frac{1}{2}$ grams on the average is of exceedingly great moment. This is especially important if the investigator believes that the point of subjective equality may give a determination of the sensitivity of the subject as well as the interval of uncertainty.[1]

One is able to make a further analysis of the relation of these differences in the points of subjective equality from the present data. In the first place, the subjects were unpracticed in the particular technique of the lifting in the beginning of the experiment, but they reached a high stage of practice during the actual experimentation. It has been shown[2] that the passing of 5,000 judgments in this sort of experiment makes the subject highly practiced. Inasmuch as we have divided the entire data for each subject into 10 fractions of 50 judgments on each comparison pair, we are able to study the effect of progressive practice on data of this sort. The values for the conclusions on this point are to be found in the next to the last columns of Tables VI.–IX. It is found that for every subject there is a tendency for the size of the differences between the points of subjective equality to increase with practice. These relations become obvious when the differences for any subject are thrown into the form of a curve. The increase of the size of the difference, however, is far from regular and is not very marked.

[1] *Cf.* F. M. Urban, 'The Method of Constant Stimuli and Its Generalizations,' *Psychol. Rev.*, XVII., 1910, 237ff.

[2] F. M. Urban, 'Der Einfluss der Uebung bei Gewichtsversuchen,' *Arch. f. d. ges Psychol.*, XXXIX., 1913, 271–311.

S. W. Fernberger, 'On the Relation of the Methods of Just Perceptible Differences and Constant Stimuli,' *Psychol. Mono.*, XIV. (Whole No. 61), 1913, 19–41.

At the end of the experiment each subject was given a series of 50 judgments, with knowledge, on each comparison pair, in an effort to determine whether the effects of this factor could be eliminated by simple instructions. The observer was informed of the divergence between his judgments in Series *A* and Series *B* and was again instructed to judge the pairs independently. In spite of this repetition of the instructions, none of the subjects shows appreciable change in his judgments. This fact becomes obvious when one examines the observed relative frequencies of the different categories of judgment—found in the last rows of Tables II.–V.—or when one examines the values for the coefficients of precision, the thresholds, the intervals of uncertainty or the points of subjective equality—found in the bottom rows of Tables VI.–IX. Hence it may be concluded that this additional knowledge has no influence upon the results. It seems that one cannot expect to eliminate this influence by simple instruction to the subject—at least to the habituated observer. The present results do not show whether proper instructions at the beginning of the experimentation, when the subject is entirely naïve to the technique, might or might not eliminate the effects of this factor.

Just what may be the nature of the processes underlying these results is not brought to light by the present experiment. All that one can say, at present, is that there is some sort of a contrast effect existing between the pairs of stimuli. It is very tempting to say that we have here a type of expectation which tends toward a reversal of judgment from one pair to the succeeding. In other words, when the subject gives a 'lighter' judgment, he is, by so doing, somehow or other 'set' toward a tendency to give a judgment of 'heavier' on the succeeding pair. And when a subject gives a 'heavier' judgment, he is, by so doing, somehow or other 'set' toward a tendency to give a judgment of 'lighter' on the succeeding pair. If this be the state of affairs, this 'set 'is not a clearly conscious process. All of the subjects were surprised when their statistical results were explained to them, and all insisted that such a form of expectation was not consciously

present. Thus the best that we can say, with our present knowledge, is that some sort of a contrast effect exists between the pairs of stimuli in the usual arrangement for the method of constant stimuli.

This contrast effect is not to be confounded with the errors of expectation present in the method of just percepitble differences.[1] In the method of just perceptible differences the factor of expectation enters just once in each series and its presence results either in the subject's reversing his judgment too soon or in his failing to reverse his judgment at the proper time. This state of affairs is due to the fact that the subject realizes that he is being presented with either an ascending or a descending series of stimuli. In the present case, on the other hand, the factor of contrast is always in the same direction and may occur as frequently as there are successive pairs of stimuli to be judged.

It seems incredible that this factor of contrast must have been present in all of the experimental work which has been done by the method of constant stimuli without its presence having been noted. None of the published curves of the psychometric functions shows forms that anyway approximate the curves shown in Figs. 1–4 of this paper.[2] The principal reason for this failure to note the factor of contrast seems to be that the curves of the psychometric functions which have actually been constructed have been formed by using the complete data for any given subject. Inasmuch as the order

[1] *Cf.* E. B. Titchener, 'Experimental Psychology,' II., Part II., 1905, 133ff.

[2] *Cf.* F. M. Urban, 'The Application of Statistical Methods to the Problems of Psychophysics.' Philadelphia, 1908.

F. M. Urban, 'Die psychophysischen Massmethoden als Grundlagen empirische Messungen,' *Arch. f. d. ges. Psychol.*, XV., 1909, 261–355; XVI., 1910, 168–227.

F. M. Urban, 'The Method of Constant Stimuli and Its Generalizations,' *Psychol. Rev.*, XVII., 1910, 229–259.

F. M. Urban, 'Ueber die Methode der mehrfachen Fälle,' *Arch. f. d. ges. Psychol.*, XVII., 1910, 367–411.

F. N. Maxfield, 'An Experiment in Linear Space Perception,' *Psychol. Mono.*, XV. (Whole No. 64), 1913.

D. Mitchell, 'The Influence of Distractions on the Formation of Judgments in Lifted Weight Experiments,' *Psychol. Mono.*, XVII. (Whole No. 74), 1914.

A. L. Ide, 'The Influence of Temperature on the Formation of Judgments in Lifted Weight Experiments' (University of Pennsylvania Thesis), Philadelphia, 1919.

of the series of stimuli have been frequently changed, so as to eliminate the possibility of a subject learning any particular order, the relations between the particular stimuli have been frequently reversed and, hence, have tended to cancel one

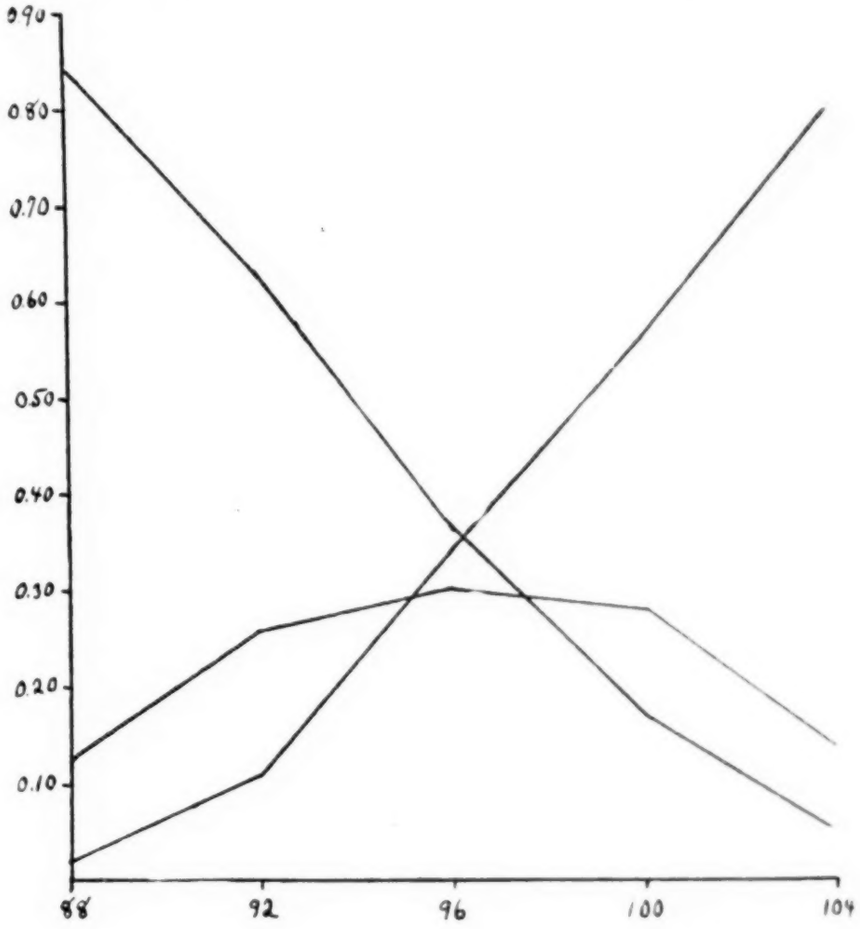

FIG. 5.

another. Ide,[1] in his recent experment, went so far as to change the order of the series after every 25 revolutions of the table. The particular order used was determined by chance. Hence, on the one hand, the grouping of a number of such orders would tend to cancel out the effect of this factor and would give a resultant curve for the complete data which closely resembles the theoretical form of the $\Phi(\gamma)$ hypothesis.

Indeed, combining the data of Series *A* and *B* for any one subject gives a resultant combined series of curves which are close—by inspection at least—to the theoretical form of the

[1] Ide, *op. cit.*, p. 8.

$\Phi(\gamma)$ hypothesis. Such a combined series of curves for Subject I. are represented in Fig. 5. These combined curves appear close to the $\Phi(\gamma)$ form for all three categories of judgment. It will also be noticed that the abscissa value for the maximum frequency of the equality judgments closely approximates the abscissa value for the intersection of the curves for the 'lighter' and 'heavier' judgments—a fact that indicates the symmetry of the functions. If these curves in Fig. 5 are compared with those in the papers quoted above, it should be remembered that we employed only five pairs of stimuli while the other investigators employed seven pairs. We have eliminated the 'tails' of the curves since they approach closely to the values of zero and unity.

It would seem evident that certain irregularities and inversions found in the published curves[1] may be more adequately explained on the basis of the presence of this factor of contrast than by explaining the facts in terms of chance variations in the results. In these cases, we believe that the different arrangements employed, although they tended toward a cancellation of the influence of contrast for some intensities of the comparison stimulus, were not so arranged as to give perfect cancellation for all of the intensities. The ideal method would be to have every pair follow every other pair an equal number of times. Assuming five pairs of weights, this condition would require 120 differently arranged series, since the permutations of 5 things taken 5 at a time is 120. But if the series were not separated, but run successively one after the other, as in the present experimental arrangement, then the relationship carries over from series to series, and the end term of the one series affects the initial pair of the next. Under these conditions, 24 series would give every paired order equally often.

A practical method for the control of this factor may be that which was unconsciously employed by former investigators: *viz*, (1) by the use of great care in the arrangement of the order of the stimuli; (2) by frequent change in the order of the stimuli so that there is a tendency for each

[1] *Cf.* especially Maxfield, *op. cit.*

weight to follow every other weight an equal number of times during the experiment. In this way the contrast factor will be in opposite directions and thus there will be a tendency for its effects to cancel.

A more systematic way of handling the influence of this factor of contrast between the pairs of weights in the method of constant stimuli, would be to present two series of stimuli to the observer, similar but with opposite objective relations and mixed one with the other—such a double series as was employed in the present investigation—and then to 'pool' the results of both series in the calculations. In this case, since the influences are in opposite directions, they tend to cancel each other. Indeed, it is found in the present results, that, if the data of the two series are combined, a single set of curves of the psychometric functions are obtained which seem to be very close to the theoretical form of the $\Phi(\gamma)$ hypothesis. (See Fig. 5.) Such a procedure would not increase either the number of results necessary for the determination of a threshold value, or the time and labor necessary for the accumulation of the data or its subsequent calculation.

Conclusions

1. There is present, in the method of constant stimuli as applied to lifted weights, a contrast effect between the pairs of stimuli. If a comparison stimulus, which is likely to fall within the interval of uncertainty, immediately follows the lightest pair, for which, of course, the judgment is usually 'lighter,' there is a strong tendency that it will be judged 'heavier.' If it follows the heaviest pair—which usually gives the judgment 'heavier'—there is an equally strong tendency that it will be judged 'lighter."

2. The presence of this factor of contrast causes a distortion of the curves for the psychometric functions for the 'lighter' and 'heavier' judgments. It has a slight effect upon the size of the interval of uncertainty and a very marked effect upon the position of the thresholds and of the point of subjective equality.

3. There seems to be a slight tendency for the effects of this factor of contrast to increase with practice.

4. It seems doubtful if simple instructions given the observer will eliminate the effects of this factor of contrast—for the habituated subject at least.

5. A method of eliminating the effects of the factor of contrast by handling the objective relations of the stimuli themselves is suggested.

NOTE ON MR. G. J. RICH'S 'STUDY OF TONAL ATTRIBUTES'

In his 'Study of Tonal Attributes' (*Amer. Journal of Psychology*, 1919, *30*, 121ff.) Mr. Rich tries to plant the dagger of fact in the bosom of my theory of sound. For one who is comfortably ensconced in the shelter of a special experimental task, it is fine sport to try a shot at the birds of theory that cross the open. But it is the bird that makes the sport. And I think it well to show that Mr. Rich has missed his mark.

Mr. Rich first dispatches me in a single brief sentence: "The idea that the volume of a tone decreases by halves as the tone becomes an octave higher implies, mathematically, that volume cannot follow Weber's law: a direct contradiction of the results obtained by Rich" (p. 127).

But both the implication and the contradiction are illusory.

Weber's law says that 'in comparing objects and observing the distinction between them, we perceive, not the difference between the objects, but the ratio of this difference to the magnitude of the objects compared.' In Mr. Rich's experiments, accordingly, the objects are the different rates of vibration that we call tones and the ratio is e.g. the differences 5, 13, 24 to the standards 275, 550, 1150 vbs. per sec. (p. 150). So, Weber might have said, in comparing octaves we note the ratio 2x to x vbs. In fact he said: "In music we apprehend the relations of tones without knowing their vibration rates . . . and in the same way we apprehend the magnitudes of sensation or of force in the comparison of weights" (v. Titchener, 'Exper. Ps. Quant.' Pt. II., xiv, xvi). Weber's theory makes no assumption of any kind about the real nature of the sensations by which the comparisons are made.

Feeling, no doubt, that his first stroke might not be fatal, Mr. Rich strikes a second time with greater force. "In particular the concept of musical interval as being the result of volume-ratio is mathematically inaccurate. If the differential limen for volume is proportional to the stimulus and if, further, the Weber-Fechner Law is accepted, then it must follow that the volume-*difference*, not the volume-ratio, is constant for the same interval at any point in the scale. This implies, of course, that the volumes of tones in octaves

cannot be halved in size as one proceeds upwards in the scale" (p. 163).

A theory must agree with the facts, surely. But if it *is* a theory, it will not agree with *any other theory*. So if Mr. Rich accepts Fechner's theory of Weber's law, and if my theory of the octave is not to be classified under that theory, then he will consider my theory wrong (perhaps without really considering it more closely).

But I do not myself think that any convincing arguments have hitherto been brought to the support of Fechner's theory. In order to show this briefly let me distinguish between functional, phenomenal, and real equality of differences between sensations.

Merely functional equality may be exemplified by some of Mr. Rich's experiments (pp. 150f.), for his observers "found considerable difficulty in describing the basis of volume-judgments." In fact they often "did not really know what a difference in volume is." And yet they gave these statistical results that "tend to follow Weber's law." We must not forget that an observer cannot really assert the presence of a just noticeable difference, but merely whether the presented sensations are different or not. It is surely obvious nowadays that a limen can only by a considerable licence be called a *fact*. *A fortiori* the question of the 'equality' of merely functional limens is still more problematical.

Functional and phenomenal equality is somewhat better exemplified in the case of easily noticeable differences, with respect to which an observer not only yields statistically constant results, but is also often conscious and sure of the presence of equality. A musical interval repeated higher or lower in the scale may still legitimately be held to illustrate this procedure (although, *e.g.*, Stumpf considers that the equalities of distance-judgments in tones do not coincide with repeated intervals. But his view is a very tentative and questionable one).

Now it is a familiar fact that the attempt has been made to gauge the real relation between functional limens by reference to the numbers of them that fill out larger differences that are phenomenally equal. The results have not spoken decisively in favor of the real equality of limens. It is difficult to see how they could be expected to do so. For the inference has no real basis at all. It contains no term involving real equality as a standard of reference. We can assume, of course, that phenomenal equality means real equality. But then the whole argument will have only the

force of this assumption. And its force seems to me to be in itself nil.

This is reinforced by the consideration that the phenomenal equality in question is an equality of difference, not an equality of substance. It is therefore a functional equality again, not of behavior, of course, or of statistical judgments, but of single judgments: *i.e.*, it is a psychical function. Hence we still have the problem of the nature of the real basis of this function. There are many possibilities, real equality being only one of these. Real proportions of all kinds are also possible.

So, if the liminal and the supraliminal series disagree, some differences (*i.e.*, one or the other series) must be unequal. If they agree, they prove nothing except this consistency.

It may be recalled that Weber demonstrated his law for short lengths of line. Mr. Rich knows that I look upon tonal volumes as 'longitudinally' variable extents. Is Weber's law then mathematically inconsistent with Weber's findings for short lines?

But is there no simpler method of solving this problem of real equality? Common sense would doubtless assume a relation of identity between a line-sensation and the thing measured as its stimulus. And there is a close connection between them. But the very experiments of Weber on these lines compel us to make an important distinction between the sensation-line and the physical line. For the experiments do not succeed unless the sensation-line is carefully maintained as a unity and is not broken up into parts by juxtaposition or overlaying. So while common sense would grant that limen differences are evidently not equal but proportional, Mr. Rich, who rejects this view, would necessarily be left again unable to say whether they are equal or different, the probability being that they are unequal.

I pointed out in my book on Sound (p. 66f.) that my inference regarding the octave might well be held to be a *real* measurement of tonal volumes. I am not sure whether the inference is perfectly convincing, but it seems to be somewhat surer than it is in what I have referred to as the common-sense view of the short line measurement. For these are not estimated relatively to one another by any psychological inference, but by physical measurement, whereas my assessment of the relations of volume between a tone and its octave is based upon purely psychological inferences regarding the facts of volumic change, pitch change, grades of fusion, etc. If the truth of the common-sense view is granted, then limens in

tonal volumes are not equal differences any more than they are for common sense in short lines. They are proportional differences.

From my theory I was prepared to meet such experimental results as Mr. Rich's; and his preliminary paper seemed to me to be confirmation of my theory. I was surprised to find that he thought his results contradicted my theory.

There is another point to urge. Even if we were to grant that all equal (and limen) differences (functional and phenomenal) are also really equal, there is nevertheless such a thing as constancy of proportion among sensory forms—phenomenal constancy. Any visual letter or pattern demonstrates this. Why should this constancy of proportion not appear in sounds also? And why not in intervals in particular? What right has Mr. Rich to assert that 'in particular, the concept of musical interval as being the result of volume-ratios is mathematically' or otherwise at all 'inaccurate'? The experiments of his that I know have no bearing on the question.

Incidentally Mr. Rich has misunderstood (p. 125f.) my paper of 1914. I never accepted Révész's Qualität as my meaning of pitch. Pitch was for me then the ordinal attribute I hold it to be. Nor is "the whole of [my] treatment of the attributes in the interest of their explanatory value for fusion" and "not in the interest of tonal analysis" (p. 127). On the contrary my most important notion of the ordinal nature of pitch was and is established largely in independence of the facts and problems of fusion. And however it may be with the real independence and the prospects of attempts to analyse tones with a perfectly detached mind I should feel sorry for any statistical treatment of the results that gets into conflict with the interests of tonal fusion. However much of fact they might contain, they would have to seek a new environment (or is it a new "attitude"?) and be born again.

HENRY J. WATT

REPLY TO DR. WATT'S "NOTE"

The first difference between Dr. Watt and myself obviously lies in the acceptance or non-acceptance of the Weber-Fechner Law. I have shown in two separate studies that the differential limen for volume is proportional to the stimulus. If, for example, the relative difference-limens for Observer *B* in the earlier study (JOURNAL OF EXPER. PSYCHOL., 1, 1914, 17) are averaged, they will be found to be .03605; hence every time that the frequency of a tone is increased by this fraction, the tone will be just noticeably smaller. There will, then, be for this observer approximately 19 just noticeable differences in every octave.[1] These just noticeable differences are to be taken, under the Weber-Fechner hypothesis, as *equal* decrements of sensation. Let us designate them by the letter a; and let us further call the volume of some tone (say, a tone of 275 vs.), m. Then the volume of a tone one octave higher (550 vs.) will be $m - 19a$; and that of a tone two octaves higher will be $m - 38a$. Thus the volumes of three tones standing in successive octaves will be: m; $m - 19a$; $m - 38a$. But Dr. Watt assures us that volume is halved for every octave that we go up the scale ('The Psychology of Sound,' 82); that is, that:

$$m = 2(m - 19a),$$

and

$$m - 19a = 2(m - 38a).$$

These two equations, however, are inconsistent with each other. There is no value of a, other than zero, for which they will both hold. If, then, my results are sound (and only further experimental work can displace them), and *if* the usual interpretation of Weber's Law is accepted, the volume of a tone is not halved as one proceeds up the scale by octaves. Any theory that so contends leads to a mathematical contradiction.

Dr. Watt has, of course, the right to prefer any law of his own to the Weber-Fechner Law. My criticism of his theory was always contingent upon the acceptance of that law, for I was aware that Dr. Watt held that the "assumption ('that the difference indicated by the words 'just perceptible difference' is . . . always equal

[1] Since $(1.03605)^{19.6} = 2$.

. . .') is unjustifiable" ('Psychology,' 23). But I was not aware that he held that limens are proportional differences. The 'Psychology of Sound' gives no hint of such a view, without which there is no justification for saying that my results were 'expected.' It is surely not enough for the theorist to say *ex post facto* that he expected from his theory a given experimental result; it is obligatory upon him to show a passage in his previous statements that, in some sort and in some measure, predicts the result. Dr. Watt has failed to adduce any evidence that he "expected" the differential limens for volume to be proportional to the stimulus. Indeed, it seems strange that, writing in January, 1917, he neglected to fortify his theory by an experimental 'confirmation' published eleven months earlier.

In comparing the limens for volume with those for lengths of line, Dr. Watt has apparently fallen into the stimulus-error. Weber's experiments showed the proportionality of the just noticeable difference to the *physical* line, not to the sensory line. In the same way I have shown that the just noticeable difference for volume is proportional to the physical sound-wave, not to the 'felt' volume. If Dr. Watt regards the attribute of volume as a characteristic of the neuro-physical process of hearing ('The Psychology of Sound,' 96f.), rather than as a characteristic of the sensory experience, I can have no quarrel with him nor he with me; for we are not talking about the same thing. If, on the other hand, Dr. Watt fails to distinguish between volume as a characteristic of stimulus and volume as a characteristic of sensation, he falls so positively into the stimulus-error that psychophysical discussion is useless. I have been and am now speaking only about 'felt' volume, about volume as an attribute of sensory experience, and not about any volumic feature of the neuro-physical process of hearing.

Dr. Watt further asserts 'that a limen can only by a considerable license be called a fact,' because it cannot be directly observed, but is always the result of statistical treatment of observations of mere equality or of difference. A limen is, of course, not a fact of direct observation; it is a value which sums up and represents a large number of facts. Scientific activity cannot limit itself to the primary facts of phenomenological observation. If it did, science would still be in the dark ages, and experiment would be impossible. All the principles of electricity would be non-existent, because no one has yet directly observed the electric current. The various laws of memory could not be admitted to our scientific thinking,

because no one has as yet directly observed the strengthening of an associative tendency with repetition or its decay with time. If Fechner may in any sense be said to have raised psychology to the rank of an exact science, it is because he introduced experimental methods of indirect observation and statistical treatment, and thereby gave to psychology the essential and manageable equivalents of whole groups of primary facts. Does Dr. Watt wish us to cast aside these experimental methods, and to return psychology to its former place as a speculative discipline?

In his last paragraph Dr. Watt accuses me of misinterpreting his 1914 paper. On page 13 of that paper he says: "Révész calls the difference of pitch within the octave 'quality' and an accompanying distinguishable difference he calls 'height.' *I accept the distinction intended* without the least hesitation, but it seems to me more correct to class the differences as differences of 'pitch' and of 'volume.' *Pitch, as I mean it, is Révész's quality*" (italics mine). How can Dr. Watt now declare that he 'never accepted Révész's "Qualität" as [his] meaning of pitch?'

Dr. Watt may, no doubt, take some interest in tonal analysis. But it is difficult to believe that that is his primary interest when we are told that: "Obviously the decision regarding these things [auditory sensations and their attributes] cannot be made on the basis of the facts themselves, but only on the basis of the solution which may independently be offered for the musical relationships of the octave" ('The Psychology of Sound,' 51), and that he 'should be sorry for any statistical treatment of the results that gets into conflict with the interests of tonal fusion. However much of fact they might contain, they would have to seek a new environment . . . and be born again.' After making these statements, upon what ground can Dr. Watt seriously deny that his treatment of the tonal attributes is primarily in the interest of fusion, and that the facts of tonal analysis are only of secondary importance in his theory?

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